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FAMILIAR ESSAYS ON
SCIENTIFIC SUBJECTS, NATURAL PHENOMENA, &c.

WITH

A SKETCH of the LIFE of MARY SOMERVILLE

BY

RICHARD A. PROCTOR, B.A. CAMB.

HONORARY SECRETARY OF THE ROYAL ASTRONOMICAL SOCIETY; AUTHOR OF
'THE SUN' 'OTHER WORLDS' 'SATURN' 'ESSAYS ON ASTRONOMY'
'THE ORBS AROUND US' ETC.

'Truths of Science waiting to be caught'—TENNYSON

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PREFACE.

THE First Series of Light Science Essays met with a success so far beyond my expectations, that I should have found in that circumstance alone a reason for adding the present volume to the series. But I have also felt a wish to publish these essays because they contain facts collected at the cost of much labour and carefully discussed,—useful, therefore, I trust, to others as well as to myself, when thus gathered into a volume.

Those who have read my former series of essays, viz. 'Light Science, Series I.,' 'The Orbs around us,' and 'Essays on Astronomy,' will perceive that even when I treat here of subjects already dealt with by me elsewhere, I have been careful to avoid the repetition of any statements, except those few without which a subject would be incomplete. For instance, it will not be easy to find in my two papers on comets in 'The

Orbs around us,' statements or reasoning repeated in the two papers on comets in the present volume.

However, for the most part, the papers in this series are distinct in subject as well as in treatment from any of my essays which have formerly appeared.

RICHD. A. PROCTOR.

LONDON: *May* 1873.

CONTENTS.

PAGE

LIFE OF MRS. SOMERVILLE 1

THE EVER-WIDENING WORLD OF STARS 15

MOVEMENTS IN THE STAR-DEPTHS 30

THE GREAT NEBULA IN ORION 53

THE SUN'S TRUE ATMOSPHERE 70

SOMETHING WRONG WITH THE SUN 93

THE SUN'S SURROUNDINGS 97

NEWS FROM HERSCHEL'S PLANET 114

THE TWO COMETS OF THE YEAR 1868:

- PART I.—BRORSEN'S COMET 140
- PART II.—WINNECKE'S COMET 156

COMETS OF SHORT PERIOD 174

THE GULF STREAM. 188

OCEANIC CIRCULATION 205

ADDENDUM IN REPLY TO DR. CARPENTER 238

THE CLIMATE OF GREAT BRITAIN 253

THE LOW BAROMETER OF THE ANTARCTIC TEMPERATE ZONE 277

LIST OF ILLUSTRATIONS.

PAGE
CHART OF THE NORTH ATLANTIC ON AN EQUAL-SURFACE PROJECTION, SHOWING THE GULF STREAM, &C. . . 210
CHART OF THE NORTHERN HEMISPHERE, SHOWING THE CURVES OF EQUAL MEAN ANNUAL TEMPERATURE AND EQUAL MID-WINTER TEMPERATURE FOR LONDON . . 256
THE SAME, SHOWING THE CURVES OF EQUAL MEAN ANNUAL TEMPERATURE AND EQUAL MID-SUMMER TEMPERATURE FOR LONDON. 258
DIAGRAM SHOWING THE ANNUAL VARIATION OF MEAN DIURNAL TEMPERATURE AT GREENWICH 270
DIAGRAM SHOWING THE BAROMETRIC PRESSURE OVER SOUTHERN HEMISPHERE 280
DIAGRAM SHOWING THE BAROMETRIC PRESSURE OVER NORTHERN HEMISPHERE . . . . *ib.*
FIGURE ILLUSTRATING A THEORY IN EXPLANATION OF THE LOW BAROMETER OF THE ANTARCTIC ZONE . . 297

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LIGHT SCIENCE FOR LEISURE HOURS

SECOND SERIES

MRS. SOMERVILLE.

MARY SOMERVILLE (*née* FAIRFAX) was born at Jedburgh on December 26, 1780, and died on November 30, 1872, at Naples, aged nearly ninety-two years. In considering her education, we have not to mention important seminaries, where skilled teachers make it their chief business to impart to others the knowledge for which they are themselves eminent, but to speak only of studies pursued in the calm of a quiet home. This, rightly understood, is perhaps the most remarkable feature of her career. There are few mathematicians so eminent as she deservedly was, in whose fame great public schools and universities do not in some degree partake. But we owe almost to accident the discovery of the powers of Mary Fairfax's mind, while the gradual development of those powers proceeded under the guidance of tutors unknown to fame, and with access only to such assistance as could be given by the friends of her own family.

Mrs. Somerville has herself described how it chanced

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that the peculiar powers of her mind came first to be recognised. She was in the habit of working at her needle in the window-seat while her brother took his lessons in geometry and arithmetic. Fortunately (in her case) the work which is regarded as most suitable to the capacity of women leaves the mind unoccupied; and consequently there was nothing to prevent Mary Fairfax from attending to the lessons intended for her brother. She gradually became interested in the subject of these lessons, and took care not only to be present regularly, but to study her brother's books in her own room. It happened that, on one occasion, young Fairfax failed to answer a question addressed to him, and his sister involuntarily prompted him. The tutor was naturally surprised that the quiet Mary Fairfax should have any ideas beyond the needlework which had apparently engaged her attention; but, being a sensible man, he was at the pains to ascertain the degree and soundness of her knowledge, and, finding that she had really grasped the first principles of mathematics, he 'took care that she should have liberty to go on in her own way.' If a boy had shown similar fitness for mathematical research, anxious attention would have been devoted to the choice of books and teachers, school and university; but the case of a girl showing such tastes seemed to be adequately met by according to her the privilege of following her own devices. We shall never know certainly, though it may be that hereafter we shall be able to guess, what science lost through the all but utter neglect of the unusual powers of Mary Fairfax's mind. We may rejoice that, through an accident, she was permitted to reach the position she actually attained; but there is scarcely a line of her writings which does not, while showing what she was, suggest thoughts of what she might have been.

While studying mathematics 'in her own way,' she found a difficulty which for a time threatened to interfere with her progress. She was unable to read the *Principia*, because she could not understand Latin. In this strait, she applied, 'after much hesitation,' to Prof. Playfair. She asked if a woman might, without impropriety, learn Latin. After ascertaining the purpose which the young lady had in view—possibly in doubt lest she might follow in the steps of Anne Dacier—Prof. Playfair told her that it would not, in his opinion, do her any harm to learn Latin in order to read the *Principia*. It is noteworthy, as having probably a bearing on the course which Mrs. Somerville's reading subsequently took, that Playfair was one of the few in this country who at that time appreciated the methods of the higher mathematical analysis, and had formed a just opinion of their power—'a power, however,' as Sir John Herschel well remarks, 'which he was content to admire and applaud rather than ready to wield.' His excellent review of the *Mécanique Céleste* probably gave (as Herschel suggests) a stronger impulse to the public mind in the direction of the higher analysis than he could have communicated by any researches of his own.

It was not, however, as a mathematician that Mrs.

Somerville first became known to the world. A subject of research, exceedingly difficult and only to be pursued successfully under very favourable conditions, was undertaken by her during the life of her first husband, Captain Greig, son of High-Admiral Greig, of the Russian Navy. She sought to determine by experiment the magnetising influence of the violet rays of the solar spectrum. 'It is not surprising,' says Sir John Herschel on this subject, 'that the feeble though unequivocal indications of magnetism which she undoubtedly obtained should have been regarded by many as insufficient to decide the question at issue.' Nevertheless it was justly regarded as a noteworthy achievement that, in a climate so unsuitable as ours, any success should have been attained in a research of such extreme difficulty. That she achieved, and, what is more, deserved success, will be inferred from the words in which Sir John Herschel indicates his own opinion of the value of her results: 'To us,' he says, 'their evidence appears entitled to considerable weight: but it is more to our immediate purpose to notice the simple and rational manner in which her experiments were conducted, the absence of needless complication and refinement in their plan, and of unnecessary or costly apparatus in their execution, and the perfect freedom from all pretension or affected embarrassment in their statement.'

In 1832 Mrs. Somerville published the work on which, in my opinion, her fame in future years will be held mainly to depend. The *Mechanism of the Heavens* was originally intended to form one of the works published by the Society for the Diffusion of Useful Knowledge, though it soon outgrew the dimensions suited for such a purpose. Indeed, it is remarkable that either Mrs. Somerville herself or Lord Brougham, at whose suggestion the work was undertaken, should suppose it possible to epitomise Laplace's *magnum opus*, or so to popularise it as to bring it within the scope of the Society's publications.

It will be well, in weighing the value of the book, to consider it first with reference to the purpose of its author, though a judgment based on that consideration alone would not be a fair one. These, then, are the words in which Mrs. Somerville presents the scope and purpose of her work :—

'A complete acquaintance with physical astronomy can only be attained by those who are well versed in the highest branches of mathematical and mechanical science : such alone can appreciate the extreme beauty of the results, and the means by which these results are obtained. Nevertheless, a sufficient skill in analysis to follow the general outline, to see the mutual dependence of the several parts of the system, and to comprehend by what means some of the most extraordinary conclusions have been arrived at, is within the reach of many who shrink from the task, appalled by difficulties which perhaps are not more formidable than those incident to the study of the elements of every branch of knowledge, and possibly overrating them by not making a sufficient distinction between the degree of mathematical acquirement necessary for making discoveries and that which is requisite for understanding what others have done. That the study of mathematics, and their application to astronomy, are full of interest, will be allowed by all who have devoted their time and attention to these pursuits; and they only can estimate the delight of arriving at truth, whether it be the discovery of a world or of a new property of numbers.'

It cannot be doubted that Mrs. Somerville here indicates her belief in the possibility of presenting her subject in a form suited to the capacities of a large number of readers, and to some extent advocates this as her object. Whether she succeeded or failed in this purpose must therefore be the first question to engage our attention. Sir John Herschel considers that she succeeded, 'for all those parts of' her subject, at least, which the work 'professes to embrace; that is to say, the general exposition of the mechanical principles employed, the planetary and lunar theories, and those of *Jupiter's* satellites, with the incidental points naturally arising out of them.' With the utmost respect for the authority of one who was so thorough a master of the subject which Mary Somerville endeavoured to popularise, I venture to express a different opinion. I find it impossible to come to any other conclusion than that, as respects the main purpose of her work, Mrs. Somerville failed entirely; though I hasten to qualify this statement by the remark that, in my opinion, success was altogether impossible. I believe, in fact, that neither Mrs. Somerville nor Sir John Herschel thoroughly apprehended the difficulty of conveying to the general reader clear ideas respecting even the elements of the subjects they severally endeavoured to expound. But I feel bound to add that Mrs. Somerville's failure, inevitable from the very nature of her task, would in any case have been brought about by the manner in which the task was accomplished. It will presently be seen that, in saying this, I am, in fact, touching on the most remarkable and distinguishing quality of Mrs. Somerville's mind.

There are two essential requisites in a treatise intended to introduce a difficult subject to general readers. First, there must be a clear apprehension of the position of such readers, of what they can and of what they cannot understand, and of the form in which what is written for them may most usefully be presented. It is not too much to say that if just ideas had been entertained by Mrs. Somerville on this point, the attempt to present the *Mechanism of the Heavens* in a popular form would never have been made. But, secondly, it is essential that in any work of the kind each statement —each sentence, in fact—should be presented in terms so precise as to be absolutely unmistakable. This is not so necessary in advanced treatises—indeed, it is too well known how large a proportion of our works on advanced science are wanting in strict precision of expression. But it is absolutely necessary in works intended to popularise science. It is a somewhat remarkable circumstance that in the *Mechanism of the*

Heavens—the boldest attempt ever made, perhaps, in this direction—not only is precision of expression not a notable feature, but, on the contrary, the most striking fault in the work is the inexactness of the language. Even Sir John Herschel, whose perfect familiarity with the subject of the work would tend to render the fault less obvious to him, was nevertheless struck by it: 'The most considerable fault we have to find,' he wrote, 'with the work before us consists in an habitual laxity of language, evidently originating in so complete a familiarity with the *quantities* concerned as to induce a disregard of the *words* by which they are designated, but which, to any one less intimately conversant with the actual analytical operations than its author, must infallibly become a source of serious errors, and which, at all events, renders it necessary for the reader to be constantly on his guard.'

These words form the penultimate sentence of Sir John Herschel's critique. I have preferred to speak first of the subject touched on, so as to pass without reservation to a more pleasing topic—the real and unquestionable value of Mrs. Somerville's chief work. And, after all, the good qualities of the work are intrinsic, while its main fault relates to a purpose which the work never could have fulfilled, no matter how carefully the fault had been avoided.

It is in this sense—regarding the work apart from its special purpose, and judging of it only as a contribution to advanced scientific literature—that we may fairly say, with Sir John Herschel, that the work is one of which any geometer might be proud. There is, indeed, ample evidence of the disadvantage under which Mrs. Somerville laboured, in the want of thorough mathematical training; but so much the more wonderful is it that she should have completely mastered her subject. Every page indicates her appreciation of the methods employed by Laplace and Lagrange. Where she does not strictly follow the *Mécanique Céleste*, she evidences a clear recognition of the purposes to be subserved by adopting a different course. I would not be understood as commending all the departures thus made; on the contrary, there are cases where it appears to me that on the whole it would have been preferable to have followed the processes of the *Mécanique Céleste* more closely, while there are others where certain more modern processes might perhaps with advantage have been introduced. But even in such instances we recognise in the course pursued by Mrs. Somerville the decision of one perfectly familiar with the subject in hand. And many of the changes must undoubtedly be regarded either as improvements, or else as altogether desirable, when the scale of Mrs. Somerville's treatise is taken into account. Amongst instances of the former kind must be classed the method employed in the investigation of the equations of continuity of a fluid: amongst instances of the latter I would specially cite the treatment of the theory of elliptic motion, in the opening chapters of the second book.

If, however, I were asked to point out the feature of this work which, in my opinion, most strikingly indicated the powers of Mrs. Somerville's mind, I should unhesitatingly select the preliminary dissertation. In this we have an abstract of the Newtonian philosophy such as none but a master-mind could have produced. Apart from its scientific value—and it has great scientific value—it is a work of great literary merit. If it is not in plan and purpose altogether original, inasmuch as it must be regarded as to some degree an abstract of Laplace's *Système du Monde*, it is nevertheless, as Herschel has well remarked, 'an abstract so vivid and judicious as to have all the merit of originality, and such as could have been produced only by one accustomed to large and general views, as well as perfectly familiar with the particulars of the subject.'

Three years after the appearance of the *Mechanism of the Heavens*, Mrs. Somerville published the work by which she is probably best known to general readers. The *Connexion of the Physical Sciences* was, I believe, written at the suggestion of Lord Brougham, as an expansion of the admirable introduction to the *Celestial Mechanism*. It is a work full of interest, not only to the student of advanced science, but to the general reader. In saying this we indicate its chief merit and its most marked defect. It is impossible to conceive that any reader, no matter how advanced or how limited his knowledge, could fail to find many most instructive pages in this work; but it is equally impossible to conceive that any one reader could find the *whole* work, or even any considerable portion, instructive or useful. The fact was that Mrs. Somerville recognised, or—which is practically the same thing—wrote as if she recognised, no distinction between the recondite and the simple. She makes no more attempt at explanation when speaking of the perturbations of the planets or discussing the most profound problems of molecular physics, than when she is merely running over a series of statements respecting geographical or climatic relations. It would almost seem as though her mind was so constituted that the difficulties which ordinary minds experience in considering complex mathematical problems had no existence for her. A writer, to whom we owe one of the best obituary notices of Mrs. Somerville which hitherto have appeared, tells us that the sort of pressure Mrs. Somerville underwent from her publisher as the earlier editions of the *Connexion of the Physical Sciences* passed through the press 'convinced her of her own unfitness for popularising science. When there was already no time to lose in regard to her proof-sheets, she had hint upon hint from Mr. Murray that this and that and the other paragraph required to be made plainer to popular comprehension. She declared that she tried very hard to please Mr. Murray and others who made the same complaint, but that every departure from scientific terms and formulas appeared to her a departure from clearness and simplicity; so that, by the time she had explained and described to the extent required, her statements seemed to her cumbrous and confused. In other words, this was not her proper work.'

Respecting her two other works, I shall merely remark that the *Physical Geography* appeared in 1848, and the *Molecular and Microscopic Science* in 1869, when she had reached the advanced age of eighty-eight years.

I may be excused for regarding Mary Somerville's life with reference rather to her astronomical and mathematical researches than to her proficiency in other branches of science. In this aspect of her career it is difficult, great as was the reputation she deservedly obtained, not to contemplate with regret those circumstances, the effects of unfortunate prejudices, whereby she was prevented from applying the full powers of her mind to the advancement of science. It is certain that no department of mathematical research was beyond her powers, and that in any she could have done original work. In mere mental grasp few men have probably surpassed her; but the thorough training, the scholarly discipline, which can alone give to the mind the power of advancing beyond the point up to which it has followed the guidance of others, had unfortunately been denied to her. Accordingly, while her writings show her power and her thorough mastery of the instruments of mathematical research, they are remarkable less for their actual value, though their value is great, than as indicating what, under happier auspices, she might have accomplished.

I have mentioned that Mrs. Somerville was twice married. By her first marriage she had one son, Mr. Woronzow Greig, since deceased. A few years after Captain Greig's death she married her cousin, Dr. Somerville, by which marriage she had three daughters, two of whom survive her. The latter years of her life (twenty-three years, we believe) were passed in Italy. It has been said by one who was well acquainted with the circumstances that 'the long exile which occupied the latter portion of her life was a weary trial to her. She carried a thoroughly Scotch heart in her breast; and the true mountaineer's longing for her native country sickened many an hour of many a tedious year. She liked London life, too, and the equal intercourses which students like herself can there enjoy; whereas, in Italy, she was out of place. She seldom met any one with whom she could converse on the subjects which interested her most; and if she studied, it could be for no further end than her own gratification. It was felt by her friends to be a truly pathetic incident that, of all people in the world, Mrs. Somerville should be debarred the sight of the singular comet of 1843; and the circumstance was symbolical of the whole case of her exile. The only Italian observatory which afforded the necessary implements was in a Jesuit establishment, where no woman was allowed to pass the threshold. At the same hour her heart yearned towards her native Scotland, and her intellect hungered for the congenial intercourse of London; and she looked up at the sky with the mortifying knowledge of what was to be seen there but for the impediment which barred her access to the great telescope at hand. With all her gentleness of temper and her lifelong habit of acquiescence, she suffered deeply, while many of her friends were indignant at the sacrifice.'

I shall venture to quote, in conclusion, some remarks by Sir Henry Holland on features of Mrs. Somerville's character and life which have been hidden from general knowledge: 'She was a woman not of science only,' he tells us, 'but of refined and cultivated tastes. Her paintings and musical talents might well have won admiration, even had there been nothing else beyond them. Her classical attainments were considerable, derived probably from that early part of life when the gentle Mary Fairfax—gentle she must ever have been—was enriching her mind by quiet study in her Scotch home. . . . A few words more on the moral part of Mrs. Somerville's character; and here, too, I speak from intimate knowledge. She was the gentlest and kindest of human beings—qualities well attested even by her features and conversation, but expressed still more in all the habits of her domestic and social life. Her modesty and humility were as remarkable as those talents which they concealed from common observation. . . . Scotland,' he justly adds, 'is proud of having produced a Crichton. She may be proud, also, in having given birthplace to Mary Somerville.'

(From *Monthly Notices of the Royal Astronomical Society* for February 1873.)

THE EVER-WIDENING WORLD OF STARS

As the science of astronomy has advanced, the ideas men have formed respecting the extent of the universe have gradually become more and more enlarged. In far-off times, when astronomers were content to judge of the conformation of the universe by the appearances directly presented to their contemplation, the ideas formed respecting the celestial bodies were singularly homely. We read that Theophrastus looked upon the Milky Way as the fastening of the stellar hemispheres, which are 'so carelessly knitted together that the fiery heavens beyond them can be seen through the spaces.' Anaximenes believed the heavens to be made of a kind of fine earthenware, and that the stars are the heads of nails driven through the domed vault formed of this material. And even Lucretius, whose views of nature were so noble, has referred without disapproval to the bizarre theory of Xenophanes that the stars are fiery clouds collected in the upper regions of air.

While the Ptolemaic system of astronomy was accepted there were no means of forming any trustworthy views respecting the extent of the stellar universe. If the earth were ever at rest we could never know how far the stars are from us; and therefore the old astronomers were free to invent whatever theories they pleased as to the scale on which the sidereal scheme is constructed. It was only when the earth was set free by Copernicus from the imaginary chains which had been conceived as holding it in the centre of the universe that it became possible to form any conception of the distances at which the stars lie from us. Indeed Tycho Brahé immediately pointed this out as an overwhelming objection against the new theory. 'Are we to suppose,' he urged, 'that the stars are placed at such enormous distances from us as to seem wholly unchanged in position while the earth sweeps round the sun in an orbit millions of miles in diameter? If this amazing theory were true, the stars would be hundreds of millions of miles from us—a view which is utterly monstrous and incredible.'

But strange as this new view appeared, it gradually gained ground. It became presently so well established that when Cassini discovered that the earth travels in a much wider orbit than Tycho Brahé had supposed—so that the stars were at once thrown many hundreds of millions of miles farther from us—astronomers still held to the new order of things. 'With Briarean arms,' as Humboldt has described their labours, the fellow-workers of Cassini thrust farther and farther away the 'heaven of the fixed stars,' until the immensity of the universe grew so great beneath their labours that new modes of expressing its dimensions had to be adopted. They were not satisfied with the obvious circumstance that the stars seem to remain unchanged in position as the earth sweeps round the sun. They tested this apparent fixity of position with instruments of greater and greater power—yet always with the same result. They made observations ten, twenty, even fifty times more exact than Tycho Brahé's, and the fact that they still detected no change of position signified nothing less than the universe of the fixed stars is ten, twenty, even fifty times farther from us than Tycho Brahé had imagined.

Thus when Sir W. Herschel began the noble series of researches amid the stellar depths which has rendered his name illustrious, the world of stars was already of inconceivably enormous extent. Yet so widely did he increase our appreciation of the vastness of the universe, that it has been thought no exaggeration to say of him that 'he broke through the barriers of the heavens:' 'Cælorum perrupit claustra,' says his monument at Upton, and every student of astronomy who has carefully examined Herschel's labours understands the justice of the expression. For, consider what Herschel did. When he began his survey of the heavens, astronomers had proved indeed that the nearest of the fixed stars lie at enormous distances from us, and some of the more advanced thinkers had begun to form noble speculations respecting the relations of the stars which lie beyond the sphere of those visible to us. But it was reserved for Sir W. Herschel to apply exact observations to the unseen star-systems. He literally gauged the celestial depths. With a telescope whose light-gathering power extended the range of vision to about eight hundred times its natural limit, he swept the whole of the northern heavens. He estimated the depth of the system of stars in every direction by a simple and natural process. For, like all great thinkers, he struck out modes of inquiry which, the moment they were presented to the world, seemed so obvious that the wonder was how they could have remained so long undetected. He said that precisely as the quantity of water passed through by the sailor's lead-line marks the depth of the sea, so the number of stars which can be seen when a telescope of given power is turned towards any part of the heavens is a measure of the depth of the sidereal system in that direction. In individual cases, indeed, the law may not be true, just as the sailor's lead-line may light on the peak of some sunken rock, and so give no true measure of the general depth of the sea in the neighbourhood. But when the average of a great number of such 'star-gaugings' is taken, then we may feel tolerably certain that on applying the simple rule devised by Herschel we shall form no inaccurate estimates of our system's extent in any direction.

Thence arose his great theory of the stellar system. He showed that our sun was but one of an immense number of suns, distributed in a region of space resembling a cloven disc in figure. When we look along the thickness of the disc we see the enormous beds of stars, which lie round us in that direction as a cloud of milky light, which so comes to form a cloven ring round the heavens. But when we look out towards the sides of the disc, where the stars are less profusely scattered, we see between them the black background of the sky.

Then Herschel extended his researches to those strange objects called the nebulæ. He showed that where astronomers had reckoned tens of these objects there were in reality thousands. And he found that a large proportion of the nebulæ can be resolved into stars. He held that these, therefore, may be looked upon as external universes, resembling that great system of stars of which our sun is a member. We need not, at this point, dwell upon the distinction which Herschel drew between nebulæ of this sort and those objects which he held (and, as we now know, justly) to be true clouds, formed of some vaporous substance, of the actual nature of which he forbore to express an opinion. Let it suffice to remark that in whatever mode those vaporous nebulæ might be supposed to be formed, it was clear to Herschel that they cannot be held to lie *necessarily* beyond the system of the fixed stars, as he held to be certainly the case with the stellar nebulæ.

Since Herschel's day a multitude of important discoveries have been made. His son, the present Sir John Herschel, carried the system of star-gaugings over the southern heavens, having first trained himself for the work by verifying Sir William's northern star-gaugings. The eminent astronomer Struve and others have applied a series of tests to the basis of Herschel's theory of the universe. Increased telescopic power has been applied to the examination of the nebulæ. And lastly, a mode of research more wonderful than the boldest pioneers of science had ventured to hope for has been applied to determine what the stars and nebulæ really are, nay even the very elements of which they are constituted.

Therefore we stand in a position so far in advance of that to which it was in Herschel's power to attain, that the attempt to modify his theories need no longer be thought to savour of undue boldness. Half a century does not pass without bringing a vast extension of knowledge, and certainly the last half century has been no exception to this rule; insomuch that could the great astronomer take his place again among us, and become cognisant of the vast strides which his favourite science has made since he left us, he would be the first to point out that many of his views require to be modified or even to be wholly abandoned.

For instance, let us consider the meaning of the following observation made by the younger Herschel: While 'sweeping' the southern heavens, this eminent astronomer noticed occasionally the existence of faint outlying streamers belonging to the Milky Way, not only irresolvable into stars, but so exceedingly distant that he could scarcely speak of them as really visible. He was *sensible* of their existence, but when the eye was turned directly upon them they vanished, insomuch that, he says, 'the idea of illusion has repeatedly arisen subsequently,' yet when he came to map down the places where these phantom star-streams had been detected, he found that they formed regular branches of the galactic system.

Now, these outlying star-streams prove first of all that the star-system is not disc-shaped, but spiral in figure. Between the stars which form the ordinary streams of the Milky Way and those which form the phantom streams, there must lie regions in which stars are either altogether wanting or strewn with much less profusion than in either the nearer or the farther stream.

But this is not the only nor the chief conclusion which may be drawn from the existence of the almost evanescent star-streams. According to Herschel's views the stars which compose those streams are only faint through enormity of distance. They may be as large as our sun, many of them perhaps far larger. And between them there may yawn distances as large as those which separate us from Arcturus or Aldebaran. Now, this being so—the outlying parts of our own sidereal system being removed so far from us as to be all but evanescent in Herschel's splendid reflector—how much greater ought to be the faintness of the sidereal systems which lie outside ours! If the nebulæ are really such systems, and made up of suns like our own, then not only ought Herschel's great reflector to fail in rendering them visible, but even Lord Rosse's noble mirror would require to be increased a hundred-fold in power before we could see them. For clearly the nebulæ, which appear as mere tiny specks upon the vault of heaven, must be very much farther away than the confines of our system, if they are comparable with it in size.

Therefore we must have 'of two things one.' Either the confines of our sidereal system are constituted very differently from the parts in our neighbourhood; or the nebulæ are constituted very differently from the sidereal system. We say, of two things one, meaning that one of the two views *must* be true; but it is plain that there is nothing to prevent both being true.

We may next come to the inquiry whether these views are severally supported by any special evidence.

Now, as to the first, it happens that the southern heavens surveyed by the younger Herschel afford evidence such as Sir William Herschel was not possessed of. The former has seen places in the southern skies where the outline of the Milky Way is so sharply defined that even in the telescope the sudden change from a background of black sky to the sprinkled light of the galaxy is not lost. One half of the field of view will exhibit the former aspect, the other the latter. Now, if we consider a cloud, or a dense flight of birds, or any cluster of objects exhibiting a well-defined outline, we see at once what that well-defined outline means. It signifies that the eye is directed along the edge or surface of a distinct cluster of objects—in one case globules of water, in another birds, and so on—and the idea is at once precluded that the eye *is within the cluster*, of whatever nature that cluster may be. Therefore the theory that the sun forms one of a system of stars spread pretty uniformly over a disc-shaped space must be given up; for were it true, the approach to the Milky Way would always be gradual.

When we add that in the southern skies the Milky Way presents the most fantastic configuration—here expanding into fan-shaped masses, there winding about in a multitude of strange convolutions; here suddenly narrowing into a bright neck or isthmus, there exhibiting a nearly circular vacancy—it becomes clear that the galaxy cannot have the figure assigned to it by Sir W. Herschel. It must consist of streams and sprays of stars at different distances. Such streams by their fantastic convolutions serve to explain all the peculiarities of the galaxy's structure.

And next, have we any evidence that the nebulæ are not really beyond the galaxy, but are mixed up with the sidereal system? It appears to me that we have.

Sir William Herschel noticed that there are places where the nebulæ are much more densely crowded than elsewhere, and he was disposed to suspect that precisely as the stars by their aggregation form the zone of the Milky Way, so there is a zone of nebulæ. But when Sir John Herschel had completed the survey of the heavens it was found that a very different law of distribution made its appearance. Instead of being collected in a zone or band around the heavens, the nebulæ are arranged in two distinct but irregular clusters, separated by a well-marked zone almost entirely free from nebulæ. *And this zone coincides almost exactly with the Milky Way.*

What are we to understand by so special an arrangement as this? A modern astronomer says it clearly proves that the nebulæ do *not* belong to the star-world; but I can see no escape from an exactly opposite view. A simple illustration will serve to exhibit the nature of the case. Suppose a person found a space of ground on which gravel was arranged in the form of a ring, and that rough stones were thickly spread over the whole space except the gravel ring, would he conclude that there was *no* association between the arrangement of the gravel and the arrangement of the stones, because few stones were to be found on or near the gravel? Would he not rather find in this peculiarity distinct evidence that there *was* some association? He would, we think, argue that the gravel had been collected into one place and the stones into another, in pursuance of *some one particular scheme*. The corresponding conclusion in the case of the stars and nebulæ would clearly be that the stars had been drawn together in one direction and the nebulæ in another, out of a common world of cosmical matter. In other words, we should look on the nebulæ as members of the same system or scheme that the stars belong to.

And here it may be asked how the conclusion thus deduced from the arrangement of stars and nebulæ can be said to tend to enlarge our views of the world of stars. On the contrary, it might be urged, the views which had prevailed before presented us with nobler conceptions of the universe. For we were able to recognise in the thousands of nebulæ which fleck the dark background of the sky, sidereal systems as noble as that of which our sun is a member; and in the existence of countless star-systems we had a spectacle to contemplate before which the human intellect was compelled to bow in its utter powerlessness and insignificance: whereas it seems as though the new views would reduce the scope of our vision to a single galaxy of stars, unless some few members of the nebular system may still be looked on as outer star-schemes.

But on a closer inspection of the views I have been maintaining, it will appear that they largely enhance our conceptions of the scale on which the world of stars is constructed. Until now it has been held that the telescopes which man has been able to construct enabled us to scan the limits of our sidereal system, and to pass so readily beyond those limits as to become sensible of the existence of thousands of other schemes as noble as our own, or nobler. But if the new views should be established, we should be compelled to recognise in the world of stars a system which our most powerful instruments are not fully able to gauge. The clusters of stars, whose splendour has so worthily excited the admiration of the Herschels, the Rosses, the Struves, and the Bonds, must be looked upon as among the glories of our own system, and indicative of the multiplied forms of structure or of aggregation to be found within its boundaries. As of late our conceptions of the wealth of the solar system have been enhanced by the discovery of numberless new objects and new forms of matter existing within its range, and co-ordinating themselves in regular relations with the earlier known members of the system, so we seem now called on to recognise in the stellar world an unsuspected wealth of material, a hitherto unrecognised variety of cosmical forms, and an extension into regions of space to which our most powerful telescopes have not yet been able to penetrate.

But now I would call attention to a peculiarity of the southern skies, which, while apparently affording conclusive testimony in favour of the new views, has unaccountably (in my opinion) been urged as an argument tending in quite another direction. There are to be seen in those skies two mysterious clouds of light, which were called, by the first Europeans who sailed the southern seas, the Magellanic clouds, and are now commonly spoken of by astronomers as the Nubeculæ. Examined by the powerful telescope of Sir John Herschel, these objects have been found to consist of small fixed stars and nebulæ, grouped together without any evidence of special arrangement, but still obviously intermixed—not merely seen projected on the same field of view.

These strange objects have given rise to many speculations; and among the definite views put forward respecting them is one recently expressed in a most valuable communication to the Royal Astronomical Society from the pen of Mr. Cleveland Abbe, an astronomer who has laboured in the sound school of the Poulkowa Observatory. Having recognised in the peculiar arrangement of stars and nebulæ above referred to an argument that the nebulæ lie beyond our system, Mr. Abbe suggests that the Magellanic clouds are two of the nearest of the nebular systems, which thus exhibit larger dimensions than their fellow-schemes.

The converse of this, which may be termed the positive theory of the Nubeculæ, is the hypothesis which may be termed the negative theory. Whatever these objects may be, astronomers have said, they are quite distinct from the sidereal system, nor are the nebulæ seen within them to be looked upon as fellows of the other nebulæ. For in the Nubeculæ we see what we recognise nowhere else, the combination, namely, of clustering groups of stars and freely-scattered nebulæ. It is the characteristic (still I am quoting the theory) of the sidereal system that where its splendours are greatest nebulæ are wanting; it is the characteristic of nebular aggregation that it withdraws itself in appearance from the neighbourhood of clustering star groups. But in the Magellanic clouds neither of these characteristics is to be recognised; therefore these objects are distinct from either system.

Nor has another argument been wanting to indicate the distinction that exists between the Magellanic clouds and the other splendours of the celestial vault. Sir John Herschel, sweeping over their neighbourhood with his 18-inch reflector, was struck with the singular barrenness of the skies around them. With that expressive verbiage which gives so great a charm to his astronomical descriptions, he forces on our attention, again and again, the poverty of the regions which lie around the Nubeculæ. 'Oppressively barren,' he describes them in one place; 'the access to the Nubeculæ on all sides is through a desert,' he says in another. And this peculiarity, thus established by the certain evidence of an observer so able and trustworthy, has been held by many to imply in the clearest and most distinct manner that there is no connection between the Nubeculæ and the stellar system.

To me the evidence afforded by the barrenness of the regions round the Magellanic clouds points irresistibly in the opposite direction. Why should some outer system, free as is assumed of all association with our own, occupy that peculiarly barren space which so attracted the attention of Sir John Herschel? But if we look on the coincidence as striking in the case of one, how much more remarkable will it appear when the only two outer systems of the sort, thus brought within our ken, are associated in this way with the most singularly barren region in the whole heavens! Surely the more natural conclusion to be drawn from the phenomenon is that the richness of the Magellanic clouds and the poverty of the surrounding districts stand to each other in the most intimate relation. Is there not reason for concluding that those districts are poor because of the action of the same process ot aggregation which has attracted within the Nubeculæ a larger share than usual of stellar and nebular glories?[1]

It need hardly be mentioned that the former argument, on which the distinction between the Nubeculæ and other celestial objects has been founded, is disposed of at once if we recognise the stellar and nebular systems as in reality forming but a single scheme. Not only so, but the Nubeculæ afford a striking argument in favour of the latter view. To return to the somewhat homely illustration made use of above. Our conceptions of the original association between the stones and the gravel arranged in the manner indicated, would certainly be strengthened, or would even be changed into absolute certainty, if we perceived in a part of the ground two heaps in which stones and gravel were intermixed. When I add that there are two distinctly marked nebular streams leading towards the Nubeculæ, as well as several well-marked star-streams tending in the same direction, the evidence of association seems greatly strengthened.

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[1] Sir William Herschel has recorded a peculiarity respecting nebulæ which is worthy of mention in connection with the facts above considered. 'I have found,' he says, 'that the spaces preceding nebulæ were generally quite deprived of stars, so as often to afford many fields without a single star.'

If these views be accepted, we shall have to look upon the world of stars as made up of all classes of clustering aggregations, besides strange wisps and sprays extending throughout space in the most fantastic convolutions. Then, also, while dismissing the idea that the nebulæ as a class are external systems, we may accept as highly probable the conclusion that some of the spiral or whirlpool nebulæ really lie far beyond the confines of our system. For we see in these objects the very picture of what the new views show our sidereal system to be. *There*, are the spiral whorls corresponding to the double ring of the Milky Way; there, are faint outlying streamers corresponding to the phantom star-streams traced by Sir John Herschel; there also, are bright single stars and miniature clusters—nay, there also, may even be recognised large knots or nodules of clustering stars, forming no inapt analogue of the Magellanic clouds.

Fraser's Magazine for July 1869.

MOVEMENTS IN THE STAR-DEPTHS.

Among the many striking contrasts between the seeming and the real suggested by the study of astronomy, there is none more startling than the contrast which exists between the apparent repose of the heavens and what is really taking place among the star depths. On a calm, clear night—

> When all the winds are laid,
> And every height comes out, and jutting peak
> And valley, and the immeasurable heavens
> Break open to their highest—

the stars seem set as emblems of eternal fixity and rest. As such they have been regarded in all ages by the poet; nor has science, so far as it has been directed to the apparent movements of the stars, taught any other lesson. It has, indeed, shown that the stars are even more steadfast than they seem, in so far as it teaches that their diurnal and annual motions are but apparent, while the great processional swaying of the star-sphere is but the reflection of the earth's gyration. More and more just, so far as these motions are concerned, has appeared the title of 'the fixed stars' assigned by astronomers to the suns which people space.

Yet the depths displayed to our view in the stillness of the calmest and clearest night are, in reality, astir with the most stupendous activity. The least of the orbs we see—some star so faint that it is only discerned by momentary gleams—is the abode of forces whose action during a single instant surpasses in effect all the forces at work upon the earth during a decade of years. All the wonderful processes taking place within and around the globe of our own sun have their analogues in that distant orb. Let it be remembered also that our sun himself presents an aspect which in no sense suggests his real condition. If we would picture him as he actually is, we must consider the uproar and tumult which prevail where, to our ordinary perceptions, all seems at perfect rest. The least movement on that glowing photosphere represents the action of forces so tremendous that they would be competent to destroy in an instant this earth on which we live. The most hideous turmoil, outvying a million-fold the roar of the hurricane or the crash of the thunderbolt, must prevail for ever in every part of the solar atmosphere. And in whatever respects other suns may differ from our own, in this at least we know that they resemble him. It is the very charter of their existence as suns—as real living centres of energy to schemes of circling worlds—that they should thus continually pulsate with their own vitality. Each is the central engine on whose internal motions the life of a system of worlds depends, and each must, with persistent activity, work out its purpose, until the fuel which supplies its forces shall be exhausted.

All the evidence as yet obtained points to the conclusion that our own sun, wonderful as is his structure and stupendous his energy, is yet very far inferior in splendour and power to most of his fellow-suns. Placed where Sirius is, the sun would appear but as a third-rate star, less bright than hundreds of the stars visible to the unaided eye. But removed to the distance of Aldebaran, or Castor, or Betelgeux, our sun would certainly not shine more brightly than the fourth-magnitude stars, while it is probable that his lustre would be so reduced that he would be barely discernible. There can be little doubt that of all the stars seen on the clearest and darkest night, there are scarce fifty which are not far larger suns than ours, and consequently the scene of more tremendous processes of change.

But when we turn from the consideration of the energy and vitality of individual stars to inquire into the movements taking place within the star system, we are yet more startlingly impressed by the contrast between the apparent rest prevailing in the star-depths and the inconceivable activity really present there. It seems incredible that all those orbs which look so still are speeding through space with a velocity compared with which every form of motion familiar to us on earth must be regarded as almost absolute rest. This appears even more surprising when we consider that during all those centuries with which history deals, during the rise and fall of the nations of antiquity, during the darkness of the Middle Ages, during the more familiar scenes of recent centuries, the stars have presented an aspect so constant that if the Chaldæan astronomers could be restored to life, they would recognise scarce any change in the positions of the stars forming the ancient constellations. Yet there are no astronomical facts more thoroughly established than those which relate to the motions of the stars. The giant orb of Sirius, exceeding our sun a thousand times in volume, Capella and Procyon, the glories of Orion, the clustered Pleiads, Arcturus, Vega, and Aldebaran, all the stars known to the astronomer, are urging their way with inconceivable velocity, each on its own course, though doubtless all these motions are subordinated to some as yet unexplained system of movements whereby all the stars of the galaxy are made to form parts of one harmonious whole.

Until lately it had only been by one method of observation that the astronomer could assure himself that these motions were taking place. That method is the simplest conceivable. If a star's place were accurately determined, either with respect to neighbouring stars or to the imaginary circles and points on the sphere which are determined by the earth's movements of rotation and revolution, then, if the star be really in motion, a change of place must in the long run manifest itself, not indeed to ordinary vision, but to the piercing scrutiny and to the yet more remarkable measuring powers of the astronomical telescope. A hundred years may elapse before the motion is measurable, yet the astronomer can none the less certainly assure himself that the motion is taking place, since he has the records of those who have gone before him, and the means of satisfying himself that those records are trustworthy.

It had long been felt, however, that there was an unfortunate gap in the evidence respecting stellar motions. The astronomer could tell how much or how little the stars were shifting on the heavens, but he could obtain no measure whatever of other motions which nevertheless must exist among the stars. If a star were receding or approaching, no trace whatever of such motion could be recognised. No instrumental means could enable the astronomer to measure the change of brightness due to the star's change of distance, since such changes must needs be infinitely small compared with the actual lustre of the star.

So that it seemed as though the astronomer must for ever remain ignorant of one most important portion of the stellar motions. All he could do, as it appeared, was to watch the aspect of the heavens, and, as it slowly changed, to infer in what way the stars were moving athwart the line of vision; and even this he could only do most imperfectly, since his knowledge of the distances of the stars is so limited that he can form but inexact notions of the rate at which the stars are so moving. They may be very far away and moving very swiftly, or they may be at a less (though still enormous) distance, and moving with a correspondingly reduced velocity.

This source of difficulty was very strikingly illustrated when the subject of the stellar motions was treated in connection with the ideas respecting the sidereal universe promulgated by Sir W. Herschel. In the hypothesis which regarded the stars as spread with a certain general uniformity through a stratum or slice of space, there was no feature which afforded any promise that by the study of the stellar motions the mysteries of the sidereal universe might be interpreted. The very basis of Sir W. Herschel's own researches into the subject is the vague supposition that it is as likely *à priori* that any given star will move in one direction as in another. Later we find Struve presenting his results in the following form: 'One may wager four hundred thousand to one that a portion of the seeming motions of the stars is due to the sun's motion, and it is an even wager (*on peut parier un contre un*) that the latter motion takes place at the rate of between 135 and 175 millions of miles per annum.' The whole question had become one of probabilities, based on more or less trustworthy assumptions. We cannot wonder greatly that, when Sir G. Airy undertook the complete re-examination of the matter thirty years ago, the result he obtained, while indicating the general probability of the inferences before obtained, nevertheless exhibited the whole problem as one needing further investigation.[1]

It will be seen presently that we cannot too attentively regard those earlier researches, if we would fully estimate the importance of the results which have recently been obtained. Let it be carefully noticed that the earlier results flowed directly from the hypotheses respecting the stars which have so long maintained their ground in our text-books of astronomy. If these hypotheses are sound, the results flowing from them, even though only based on the general principles of probability as applied to those hypotheses, might be expected to be somewhat near the truth. If, on the contrary, an independent and trustworthy series of results should show that those earlier results are not correct—are indeed very far from correctness—then *pro tanto* the hypotheses which led to those earlier results would be invalidated.

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[1] This part of my subject is fully discussed in a paper called 'The Sun's Journey through Space,' which appeared in *Fraser's Magazine* for September 1869, and will be found among my 'Essays on Astronomy.'

Let it then be clearly understood that, according to the results in question, the stars were held to be in motion at rates comparable in general with the velocity of our sun, this velocity being estimated at about four and three-quarter miles per second. We do not include here the result that the sun is moving towards Hercules, because that may be regarded as established, whatever opinion we may form as to the distribution of the stars in space.

Before proceeding to indicate the bearing of recent observations on these theoretical conclusions, I would invite some degree of attention to the circumstance that the view I am here advancing as to the bearing of new facts on the old hypothesis, is not a new one framed to account for the new facts in a way agreeing with my own theories respecting the stars. More than thirteen years ago in 'Fraser's Magazine,' and earlier still in the Proceedings of scientific societies, I indicated my belief that the real facts are precisely such as have now been demonstrated.

Already when I so wrote, promise had been afforded that the astronomer might come in time to know,[1] not merely whether certain stars are approaching or receding, but at what rate (in miles per second) these motions are taking place. I need not here enter into an explanation of the method by which this was to be accomplished, inasmuch as a full account of the principle on which the method is based is given in the paper called 'News from Sirius,' in my Essays on Astronomy. Suffice it to say, that it depends on the observed displacement of some known dark line in the rainbow-tinted streak forming the spectrum of a star, and that when such a line is displaced towards the red end of the spectrum it is known that the star is receding, while when the displacement is towards the violet end it is known that the star is approaching.

Dr. Huggins, our great spectroscopist, had successfully applied this method to the star Sirius, and he had found that that star is receding from the earth at the rate of upwards of twenty-five miles per second. But Sirius was the only star which could then be examined by this method. The light of Sirius exceeds more than five times that of the next star in order of brightness, at least of those visible in our hemisphere; and with the instrument then at Dr. Huggins' disposal (his own eight-inch refractor) it was found impossible to see the dark lines of any other star-spectrum with a spectroscope dispersive enough to give any measurable displacement of the lines.

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[1] See the closing words of the last paragraph but three in the essay mentioned.

But the importance of the inquiry (as well as of those other spectroscopic researches in which Dr. Huggins had been so successful) was manifest to our scientific societies; and accordingly a large sum was granted by the Royal Society for the construction of a refracting telescope, fifteen inches in aperture, to enable Dr. Huggins to extend his researches to the leading stars of our northern heavens. This fine instrument was ready for use in the spring of 1872, and before many weeks had passed Dr. Huggins had obtained results of surpassing interest and importance. He had recognised motions of recession and approach in no less than thirty stars, and had traced laws before unknown in the phenomena of these stellar motions.

One of the most striking features in the series of star-motions observed and measured by Dr. Huggins is the amazing velocity with which some of the stars are moving. Astronomers had ascertained that Sirius is moving athwart the line of vision much more rapidly than the sun is travelling through space. But Sirius is so exceptional both in his brightness and in his estimated bulk, that his enormous velocity did not appear altogether surprising. It did not lead the generality of astronomers to consider that the sun's velocity and the average velocity of the stars had been greatly underestimated. But now we learn from a method of research which is far more trustworthy than any applied to the measurement of thwart motions, that some of the stars are moving from or towards the earth with a velocity far exceeding that of Sirius. If we take the thwart motion of Sirius at twenty-five miles per second, and his motion of recession at twenty miles (this being the value assigned by the latest and best measurements), we find for this absolute motion the amazing velocity of about thirty-two miles per second. But Dr. Huggins finds that Arcturus is receding from the sun at the rate of 55 miles per second, Vega at the rate of about 50 miles, Arided (the chief brilliant of the Swan) at the rate of 39 miles, Pollux 49 miles, and Dubhe of the Great Bear at the rate of from 46 to 60 miles per second. Beside such motions as these, our sun's estimated velocity of about 4¾ miles per second, which had seemed so imposing when it was considered that he bore with him at this enormous rate his whole family of planets, sinks into relative insignificance. We here recognise stellar rates of motion nearly equalling that at which our earth circuits around the sun. But a velocity which, considered with reference to a minute orb like the earth, is intelligible, becomes altogether startling in the case of orbs like Arcturus and Vega, which undoubtedly exceed our own sun many times in volume. I use the word 'intelligible' with a purpose; for I am not considering here what is conceivable or the reverse. We can in reality *understand* why the earth should be possessed of the velocity she actually displays. We know that the sun's attraction is competent to generate such a velocity, or a much greater velocity. But in the case of a star these swift motions cannot be thus explained. The stars are too far apart to be so influenced by their mutual attractions that great velocities would be generated. And thus the thoughtful mind cannot but recognise in the stellar motions a subject of contemplation far more impressive than the subordinate though even swifter motions of the Earth, Venus, or Mercury. Whence sprang that amazing energy which is represented by the proper motions of the suns? If we admit the possibility that forces of eruption or expulsion could account for the observed motions, we shall have to answer the startling question, Of what order are the orbs whence the giant suns are expelled? and the yet more difficult questions, Where are these orbs? and, How is it that, inordinately large though they must be, we are yet unable to distinguish them from ordinary suns? If, on the other hand, we prefer to regard the stellar velocities as generated by the attractions of larger orders of bodies than the stars (as planetary velocities may be regarded as generated by their parent suns), we still have the last two questions to answer; and, so far as can be judged, these questions are at present unanswerable.[1]

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[1] In passing, however, I would venture to touch on this question of central suns, or of central but opaque orbs round which the stars may revolve, in order to remove a very prevalent misconception. It seems to be commonly supposed that we cannot imagine such orbs to lie far enough away to account for their not being discernible

Another striking feature in the results announced by Dr. Huggins is the absence of any systematic agreement between the stellar motions he has recognised, and the motion of our sun towards Hercules. It is manifest that if our sun were alone in motion, the actual rates of approach and recession of all the stars in the heavens would be at once determined when the rate of the sun's motion was determined. If, for example, he were moving at the rate of twenty miles per second towards the star Lambda of Hercules, he would be approaching every star lying in that direction at the same rate; he would be receding from all stars lying in the opposite direction at the same rate; and he would be approaching or receding from stars lying in opposite directions at a less rate (readily calculable). A certain half of the heavens would contain all the stars which the sun was approaching; the other half would contain all the stars from which he was receding; and the circle separating these halves would mark the place of stars which the sun was neither receding from

either as orbs of light or by hiding more distant stars, without depriving them of the attractive influence necessary to sway the motions of the stars. This, however, is not the case. An orb looking as bright as Sirius, but ten times as far away, if of equal density and inherent brightness, would be a thousand times more massive, while the effect of distance would only be to reduce its attraction one hundred times. It would, therefore, attract our sun ten times as strongly as Sirius actually does. In like manner, an orb one hundred times as far away as Sirius, but so large as to appear as bright, would attract our sun one hundred times as strongly, and so on. So that it cannot be positively asserted that among the stars visible to us there may not be the central sun of the sidereal scheme—inordinately large and massive compared with the rest, but reduced by distance to the same order of brightness.

nor approaching. But nothing of this sort can be recognised in the observed stellar rates of approach and recession. Sirius (which lies nearly opposite to Hercules) is receding at the rate of about 20 miles per second; but Vega (which lies close to Hercules), instead of approaching at about the same rate, is actually approaching at the rate of about 50 miles per second. Castor, which is very near the border line between the two hemispheres just mentioned, and should therefore neither be approaching nor receding, is in fact receding at the rate of about 25 miles per second: while Pollux, though similarly placed, is approaching the sun at the rate of about 49 miles per second. Again, of the seven bright stars forming Charles's Wain, six are approaching (five of them at the rate of about 20 miles per second), while the seventh is receding at a rate probably exceeding 50 miles per second.

Thus we see that the sun cannot be regarded as an orb moving within the scheme of stars, and by his own movement causing the chief apparent motions of the surrounding orbs. His motion is but part of a grand scheme of motions, whose laws are as yet unknown to us. We may recognise in the method of research which has now been so successfully applied the sole means of determining what those laws may be. We can now tell the very rate, in miles per annum, at which the suns are approaching or receding from us; and though we have no reason for believing that our sun occupies in any sense a central position—so that we have yet to learn at what rate and in what way the stars move around the true centre of their system—yet it is far from unlikely that if we can but ascertain the motions of a sufficient number of stars, we shall have the means of judging where the centre lies round which these motions are taking place.

The astronomer may well look with doubt, however, on the efforts which are being made to solve this stupendous problem. If we may judge from the analogy of our own solar system, we can see that in the far more complicated scheme of the stars there must exist innumerable features to perplex the observer. If we imagine a being placed in the midst of the solar system, and enabled to study the various apparent motions visible from his stand-point, and if we further suppose him gifted with the power of measuring the rate at which the various orbs are approaching him or receding from him, then we know that if his scrutiny were but continued long enough, he could not fail to recognise the laws which exist within that system and regulate all those motions. Where at first all had seemed confusion, our imaginary observer would recognise in the course of time a beautiful harmony; motions which had appeared discordant would be found to be in reality subordinated into one grand scheme. But if we suppose our observer to occupy his imaginary stand-point for a few hours, or even for a few days only, how imperfect would be his ideas of the harmony of the celestial motions! He would see the primary planets moving apparently in diverse directions and at inconsistent rates; the secondary planets apparently travelling with non-accordant motions and on different paths; the asteroids would perplex him by their wide range of apparent distribution; meteoric systems would appear to conform to no recognisable law; and the movements of comets would seem altogether inexplicable.

Yet the terrestrial observer of the infinitely more complicated sidereal system is in reality even less favourably circumstanced than our imaginary observer of the planetary scheme. The motions which come within his ken are more minute, compared with the real dimensions of the stellar paths, than the motion of Saturn or Jupiter in a single second compared with the wide orbits traversed by these planets. We cannot tell whether the observed motion of a star is that by which it is carried on some vast independent orbit; or is its motion within some subordinate scheme; or, lastly, is for the most part due to the sun's own motion within the sidereal system. When we see the stars of the same constellation carried in different directions, we cannot tell whether the real motions are diverse in character, or whether the diversity is but apparent, like the apparent advance and retrogression of planets which, nevertheless, are travelling in a common direction around a common centre.

But precisely because the difficulties which surround the problem of the stellar motions are so stupendous, we must so much the more carefully examine every feature which observation may reveal to us. To do otherwise were to abandon the problem as altogether hopeless.

Now it cannot but be recognised that in this respect the new method of research is peculiarly promising. For whereas all former methods have dealt only with apparent motion, this method tells us of the real rate of stellar displacements. We have seen how it has disposed of the inferences which had been formed as to the sun's velocity, and the average velocities of stellar motion; let us inquire what has been its bearing on the views of astronomers respecting the stellar universe regarded as a scheme or system.

Other methods of dealing with the motions of the stars had related chiefly to the question of the sun's journey through space, until Mädler was led to inquire whether the motions of the stars might not afford the means of determining where the centres of the stellar system may lie. Limiting his range of inquiry, in the first instance, by certain preliminary considerations, he proceeded to examine the direction of the apparent stellar motions in a particular region of the heavens. It seemed likely to him that the centre of the universe would be near the Milky Way, and probably on that band of conspicuous stars which extends over the Greater Dog, Orion, the Bull, Perseus, and Cassiopeia. Still further, he reasoned that if the sun is circling around the central orb, this body must lie on a line square to the sun's path; so that if we imagine a line extending from a point in the heavens *from* which the sun is travelling to the point *towards* which he is travelling, then the central orb must lie somewhere on or near to a plane through the sun and square to that line. Now such a plane would cut the Milky Way in two places, one in the northern heavens in Perseus, the other in the southern heavens between the Altar and the Centaur. Mädler further indicates reasons for believing that the centre of the sidereal universe lies towards the northern region of the Milky Way. Lastly, seeing that not far from the northern region there is a remarkable star cluster, the Pleiades, he was led to examine the region around the Pleiades for those signs which he thought likely to exist towards that part of the heavens where lies the centre of the sidereal universe. We do not enter here into a consideration of the reasoning which led Mädler to conclude that in that part of the heavens the stars would all appear to be moving in the same general direction, for they are rather recondite. That, however, was his anticipation; and as he found that the stars in the constellation Taurus are nearly all moving southwards, he was satisfied that he had not been mistaken in setting the Pleiades as the central region of the universe, and the star Alcyone, the brightest of the Pleiades, as the central orb around which all the stars revolve.

Now to such a problem as this—a problem whose grandeur cannot but be recognised even by those who reject the conclusions adopted by Mädler—the new method of research is applicable with peculiar force. For instance, if the stars of Taurus are circling round a particular orb also in Taurus, it will be manifest, on a moment's consideration, that they can have only a slight motion either of recession or approach with respect to the sun. When from our station on the earth we see Venus or Mercury nearly in the same direction as the sun, we know that at the moment either planet has only a thwart motion, being then either at its greatest or least distance from us. So that if the new method were applied to stars in Taurus, and showed that swift motions of recession or approach are there in progress, it would at once dispose of the attractive but too speculative theory of the German astronomer.

This has not yet been accomplished; in fact, since Dr. Huggins' instrument was mounted and in order, the constellation Taurus has not been well-placed for observation by the new method. But in the meantime, evidence of the most convincing nature has been obtained to show that Mädler's theory is unsound.

We have seen that the theory was based, in the main, on a certain general community of apparent motion among the stars in Taurus. Mädler took it for granted that this community of motion is exceptional. It did not occur to him to examine the motions of stars in other parts of the heavens, to see whether perchance a like feature might not present itself elsewhere.

Having been myself led by other inquiries than Mädler's to the conclusion that the stellar motions might afford useful information as to the structure of the heavens, I thought it desirable to make a chart showing all the known stellar motions in such a way that wherever a community of direction exists it would be at once apparent in the chart. Little arrows, affixed to the star-discs on the map, showed by their direction and length the nature and amount of the stellar thwart motions. When the map was completed it was easy to see that the community of motion in Taurus was only one instance, and by no means the most striking which could be recognised, of a phenomenon which I have since called *star-drift*. Certain sets of stars are seen to be moving athwart the heavens, nearly in the same direction, and nearly at the same rate, in such sort as to show that they form distinct families of suns, travelling onwards—each family as a single group—through the celestial spaces.

If this view is just, Mädler's theory is at once shown to be unsound; since the stars in Taurus thus appear as simply a drifting family of stars, one among several such families.

All that was required to make the proof convincing was, that one of these sets of drifting stars should be shown to be either approaching the earth or receding from it as a single group.

Now, among the instances of star-drift there was one in the Great Bear which presented some very striking features. Five stars in this constellation, known as Beta, Gamma, Delta, Eta, and Zeta, were seen to be travelling, not merely at the same rate and in the same direction, but on a course precisely opposite to that which they would have had if their apparent motion had been due to the sun's motion in space. Moreover, all these stars are large and conspicuous; while one of them, Zeta, is distinguished by having two companions, one very close to it, and the other so far away that its motion around Zeta is only completed (according to Mädler's computation) in a period of about 2,000 years; so that, if all the five large stars form a single system, the cyclic revolutions of the system must require millions of millions of years for their completion.

I selected this family of stars as affording a convenient means of testing (crucially) the accuracy of my theory of star-drift. If that theory is just, all these stars must be either approaching or receding at a common rate. If the theory is unsound, the chances are enormous against their possessing a common motion of approach or recession. I expressed a strong feeling of confidence that whenever Dr. Huggins applied the new method of research to these stars, he would find that they are either all approaching or all receding, and at one and the same rate. When I expressed this opinion, I knew that before many months had passed the matter would be decided one way or the other.

Nothing could be more complete than the confirmation of my views by Dr. Huggins' observations. In his table of stellar motions, Dr. Huggins *brackets together* the five stars in question as possessing a common motion of recession at the rate of about twenty miles per second. Moreover, he finds, from the nature of their spectra, that they are all alike in physical constitution.

It is hardly necessary to insist upon the importance of this result. It proves, first, that in this instance—and therefore presumably in the other instances—of *apparent* star-drift, there is a distinct family or group of stars, travelling bodily onwards amidst the star-depths. It is shown that the motions taking place within this star-family are small compared with the common motion of the group. It can be inferred that the group is relatively isolated, since otherwise we should find other stars in the Great Bear sharing in the motion of these five; and also, if there had been a disturbing orb at a moderate distance from the group, the members of the family would ere this have lost their uniformity of motion. Whatever may be the centre around which these five stars are moving as a single group, the distance of that centre must exceed enormously the dimensions of the group, precisely as the distance of the sun from Jupiter's satellite family enormously exceeds the dimensions of that system. Yet the distances separating the stars of the Great Bear are themselves amazingly vast. The distance between Beta and Zeta of the Great Bear cannot be less than 100,000 times the distance separating our earth from the sun, and is probably far vaster. What then must be the distance of the centre of motion, as seen from which this enormous space is reduced to an almost evanescent arc!

It seems not unlikely that we ought to regard the family of stars here recognised as bearing the same general relation to the stellar universe (or to that portion of it to which our sun belongs) that a group of meteors bears to the solar system. All the drifting star-families may not indeed travel around one and the same centre; *or* there may be no true centre, but only a central region, round which these movements take place; but it is impossible to consider thoughtfully any instance of community of stellar motions without feeling that it implies a common influence affecting in the same or nearly the same way each member of the drifting star-family. If there is but one such centre, whether it be a single orb or a central region of thickly clustering stars, there now seems to be at least a possibility that we may find where this centre lies. When only a few more star-families have been recognised, and their motions of approach or recession determined, it will be a problem of no inordinate difficulty to deduce the position in space of the regions round which these motions are taking place, or else to prove (which would equally be a solution of the problem now before us) that no such region exists, and that the stars drift around more centres than one.

Whatever success may attend the efforts made to explain the stellar motions, there can be no doubt that the problem is well worthy of the most thorough investigation. There is, indeed, something startling in the thought that man, placed as he is on a tiny orb—an orb rotating swiftly on its axis, carried swiftly round the sun, and borne along with him in his swift motion through space—man, shortlived and weak, and unable by his unaided vision to perceive a thousandth part of the star-system, should yet attempt (and not unhopefully) to master the secret of its structure and motions. It may be that what has hitherto been done is but the beginning of the series of labours by which, if ever, that end will be accomplished; or it may be that we are nearer to the mastery of the problem than we at present imagine; but, in any case, there is but one course by which success can be achieved. Piece by piece the facts on which our reasoning is to depend must be gathered together; while at every stage of the inquiry the full meaning of observed facts must be as far as possible evolved. Success will not be obtained by observation alone, nor by theorising alone; but by that combination only of observation and theory to which we owe all the most important discoveries hitherto effected by astronomers.

Fraser's Magazine for November 1872.

THE GREAT NEBULA IN ORION.

During the first four months of the year the constellation Orion is very favourably situated for observation in the evening. This magnificent asterism is more easily recognised than the Great Bear, Cassiopeia's Chair, or the fine festoon of stars which adorns the constellation Perseus. There is, indeed, a peculiarity about Orion which tends considerably to facilitate recognition. The other constellations named above, gyrate round the pole in a manner which presents them to us in continually varying positions. It is not so with Orion. Divided centrally by the equator, the mighty hunter continues twelve hours above and twelve hours below the horizon. His shoulders are visible somewhat more, his feet somewhat less, than twelve hours. When he is in the south he is seen as a giant with upraised arms, erect, and having one knee bent, as if he were ascending a height. Before him, as if raised on his left arm, is a curve of small stars, forming the shield, or target of lion skin, which he is represented as uprearing in the face of Taurus. When Orion is in the east, his figure is inclined backwards; when he is setting, he seems to be bent forwards, as if rushing down a height; but he is never seen in an inverted position, like the northern constellations.

And we may note in passing that the figure of Orion, as he sets, does not exactly correspond with the image presented in that fine passage in *Maud*:

I arose, and all by myself, in my own dark garden ground,
Listening now to the tide, in its broadflung shipwrecking roar,
Now to the scream of a maddened beach dragged down by the wave,
Walked in a wintry wind, by a ghastly glimmer, and found
The shining Daffodil dead, and Orion low in his grave;

and again, towards the end of the poem:

It fell on a time of year
When the face of night is fair on the dewy downs,
And the shining Daffodil dies, and the charioteer
And starry Gemini hang like glorious crowns
Over Orion's grave low down in the West.

I would not, however, for one moment be understood as finding fault with these passages of Tennyson's finest poem. Detached from the context, the image is undoubtedly faulty; but there is a correctness in the very incorrectness of the image, placed as it is in the mouth of one

Raging alone as his father raged in his mood;

brooding evermore on his father's self-murder:

On a horror of shattered limbs . . . .
Mangled and flattened and crushed.

Let us pass on, however, to the subject of our paper.

Beneath the three bright stars which form the belt of Orion are several small stars, ranged, when Orion is in the south, in a vertical direction. These form the sword of the giant. On a clear night it is easy to see that the middle star of the sword presents a peculiarity of appearance: it shines as through a diffused haze.

In an opera-glass this phenomenon is yet more easily recognisable. A very small telescope exhibits the cause of the peculiarity, for it is at once seen that what seemed a star is in reality a mass of small stars intermixed with a diffused nebulosity.

It is a very remarkable circumstance that Galileo, whose small telescope, directed to the clear skies of Italy, revealed so many interesting phenomena, failed to detect

> That marvellous round of milky light
> Below Orion.

It would not, indeed, have been very remarkable if he had simply failed to notice this object. But he would seem to have directed his attention for some time especially to the region in the midst of which Orion's nebula is found. He says: 'At first I meant to delineate the whole of this constellation; but on account of the immense multitude of stars—being also hampered through want of leisure—I left the completion of this design till I should have another opportunity.' He therefore directed his attention wholly to a space of about ten square degrees, between the belt and sword, in which space he counted no less than four hundred stars. What is yet more remarkable, he mentions the fact that there are many small spots on the heavens shining with a light resembling that of the Milky Way (*complures similis coloris areolæ sparsim per æthera subfulgeant*); and he even speaks of nebulæ of this sort in the head and belt and *sword* of Orion. He asserts, however, that by means of his telescope, these nebulæ were distinctly resolved into stars—a circumstance which, as we shall see presently, renders his description wholly inapplicable to the great nebula. Yet the very star around which (in the naked-eye view) this nebula appears to cling, is figured in Galileo's drawing of the belt and sword of Orion!

It seems almost inconceivable that Galileo should have overlooked the nebula, assuming its appearance in his day to have resembled that which it has at present. And as it appears to have been established that, if the nebula has changed at all during the past century, it has changed very slowly indeed, one can scarcely believe that in Galileo's time it should have presented a very different aspect. Is it possible that the view suggested by Humboldt is correct—that Galileo did not see the nebula because he did not *wish* to see it? 'Galileo,' says Humboldt, 'was disinclined to admit or assume the existence of starless nebulæ.' Long after the discovery of the great nebula in Andromeda—known as 'the transcendently beautiful queen of the nebulæ'—Galileo omitted all mention in his works of any but starry nebulæ. The last-named nebula was discovered in 1614 by Simon Marius, whose claims to the discovery of Jupiter's satellites had greatly angered Galileo, and had called forth a torrent of invective, in which the Protestant German was abused as a heretic by Galileo, little aware that he would himself before long incur the displeasure of the Church. If we could suppose that an unwillingness, either to confirm his rival's discovery of a starless nebula, or to acknowledge that he had himself fallen into an error on the subject of nebulæ, prevented Galileo from speaking about the great nebula in Orion, we should be compelled to form but a low opinion of his honesty. It happens too frequently that

> The man of science himself is fonder of glory, and vain—
> An eye well practised in nature, a spirit bounded and poor.

That Hevelius, the 'star-cataloguer,' should have failed to detect the Orion nebula is far less remarkable; for Hevelius objected to the use of telescopes in the work of cataloguing stars. He determined the position of each star by looking at the star through minute holes or pinnules, at the ends of a long rod attached to an instrument resembling the quadrant.

The actual discoverer of the great nebula was Huyghens, in 1656. The description he gives of the discovery is so animated and interesting that we shall translate it at length. He says:

'While I was observing the variable belts of Jupiter, a dark band across the centre of Mars, and some indistinct phenomena on his disc, I detected among the fixed stars an appearance resembling nothing which had ever been seen before, so far as I am aware. This phenomenon can only be seen with large telescopes, such as I myself make use of. Astronomers reckon that there are three stars in the sword of Orion, which lie very close to each other. But as I was looking, in the year 1656, through my 23-feet telescope, at the middle of the sword, I saw, in place of one star, no less than twelve stars—which indeed is no unusual occurrence with powerful telescopes. Three of these stars seemed to be almost in contact, and with these were four others which shone as through a haze, so that the space around shone much more brightly than the rest of the sky. And as the heavens were serene and appeared very dark, there seemed to be a gap in this part, through which a view was disclosed of brighter heavens beyond. All this I have continued to see up to the present time [the work in which these remarks appear—the *Systema Saturnium*—was published in 1659], so that this singular object, whatever it is, may be inferred to remain constantly in that part of the sky. I certainly have never seen anything resembling it in any other of the fixed stars. For other objects once thought to be nebulous, and the Milky Way itself, show no mistiness when looked at through telescopes, nor are they anything but congeries of stars thickly clustered together.'

Huyghens does not seem to have noticed that the space between the three stars he described as close together is perfectly free from nebulous light—insomuch that if the nebula itself is rightly compared to a gap in the darker heavens, this spot resembles a gap within the nebula. And indeed it is not uninteresting to notice how comparatively inefficient was Huyghens' telescope, though it was nearly eight yards in focal length. A good achromatic telescope two feet long would reveal more than Huyghens was able to detect with his unwieldy instrument.

Dominic Cassini soon after discovered a fourth star near the three seen by Huyghens. The four form the celebrated *trapezium*, an object interesting to the possessors of moderately good telescopes, and which has also been a subject of close investigation by professed astronomers. Besides the four stars seen by Cassini, there have been found five minute stars within and around the trapezium. These tiny objects seem to shine with variable brilliancy; for sometimes one will surpass the rest, while at others it will be almost invisible.

After Cassini's discovery, pictures were made of the great nebula by Picard, Le Gentil, and Messier. These present no features of special interest. It is as we approach the present time, and find the great nebula the centre of quite a little warfare among astronomers —now claimed as an ally by one party, now by their opponents—that we begin to attach an almost romantic interest to the investigation of this remarkable object.

In the year 1811, Sir W. Herschel announced that he had (as he supposed) detected changes in the Orion nebula. The announcement appeared in connection with a very remarkable theory respecting nebulæ generally—Herschel's celebrated hypothesis of the conversion of some nebulæ into stars. The astronomical world now heard for the first time of that self-luminous nebulous matter, distributed in a highly attenuated form throughout the celestial regions, which Herschel looked upon as the material from which the stars have been originally formed. There is an allusion to this theory in those words of the Princess Ida:

> There sinks the nebulous star we call the Sun,
> If that hypothesis of theirs be sound.

And in the teaching of 'comely Psyche':

> This world was once a fluid haze of light,
> Till toward the centre set the starry tides,
> And eddied into suns, that wheeling cast
> The planets.

Few theories have met with a stranger fate. Received respectfully at first on the authority of the great astronomer who propounded it—then in the zenith of his fame—the theory gradually found a place in nearly all astronomical works. But, in the words of a distinguished living astronomer, 'The bold hypothesis did not receive that confirmation from the labours of subsequent inquirers which is so remarkable in the case of many of Herschel's other speculations.' It came to pass at length that the theory was looked upon by nearly all English astronomers as wholly untenable. In Germany it was never abandoned, however, and a great modern discovery has suddenly brought it into general favour, and has in this, as in so many other instances, vindicated Herschel's claim to be looked upon as the most clear-sighted, as well as the boldest and most original of astronomical theorisers.

Herschel had pointed out various circumstances which, in his opinion, justified a belief in the existence of a nebulous substance—fire-mist or star-mist, as it has been termed—throughout interstellar space. He had discovered and observed several thousand nebulæ, and he considered that amongst these he could detect traces of progressive development. Some nebulæ were, he supposed, comparatively *young*; they showed no signs of systematic aggregation, or of central condensation. In some nebulæ he traced the approach towards the formation of subordinate centres of attraction; while in others, again, a single centre began to be noticeable. He showed the various steps by which aggregation of the former kind might be supposed to result in the formation of star-clusters, and condensation of the latter kind into the formation of conspicuous single stars.

But it was felt that the strongest part of Herschel's case lay in his reference to the great nebula of Orion. He pointed out that amongst all the nebulæ which might be reasonably assumed to be star-systems, a certain proportionality had always been found to exist between the telescope which first detected a nebula and that which effected its resolution into stars. And this was what might be expected to happen with star-systems lying beyond our galactic system. But how different is this from what was seen in the case of the Orion nebula. Here is an object so brilliant as to be visible to the naked eye, and which is found on examination to cover a large region of the heavens. And yet the most powerful telescopes had failed to show the slightest symptom of resolution. Were we to believe that we saw here a system of suns so far off that no telescope could exhibit the separate existence of the component luminaries, and therefore (considering merely the observed extent of the nebula, which is undoubtedly but a faint indication of its real dimensions) so inconceivably enormous in extent that the star-system of which our sun is a member shrinks into nothingness in comparison? Surely it seemed far more reasonable to recognise in the Orion nebula but a portion of our galaxy,—a portion very vast in extent, but far inferior to that 'limitless ocean of universes' presented to us by the other view.

And when Sir W. Herschel was able, as he thought, to point to changes taking place within the Orion nebula, it seemed yet more improbable that the object was a star-system.

But now telescopes more powerful than those with which the elder Herschel had scanned the great nebula were directed to its examination. Sir John Herschel, following in his father's footsteps, applied himself to the diligent survey of the marvellous nebula with a reflecting telescope eighteen inches in aperture. He presented the nebula to us as an object of which 'the revelation of the ten-feet telescope was but the mere rudiment.' Strange outlying wisps and streamers of light were seen, extending far out into space. Yet more strange seemed the internal constitution of the object. So strange, indeed, did the nebula appear, 'so unlike what had hitherto been known of collections of stars,' that Sir John Herschel considered the evidence afforded by its appearance as sufficient to warrant the conclusion of a non-stellar substance.

But this eminent astronomer obtained a yet better view of the great nebula when he transported to the Cape of Good Hope an instrument equal in power to that which he had applied to the northern heavens. In the clear skies of the southern hemisphere the nebula shines with a splendour far surpassing that which it has in northern climes. It is also seen far higher above the horizon. Thus the drawing which Sir J. Herschel was able to execute during his three years' residence at the Cape is among the best views of the great nebula that has ever been taken. But even under these favourable circumstances, Sir John records 'that the nebula, through his great reflector, showed not a symptom of resolution.'

Then Lassell turned his powerful mirror, two feet in diameter, upon the unintelligible nebula. But though he was able to execute a remarkable drawing of the object, he could discern no trace of stellar constitution.

In 1845 Lord Rosse interrogated the great nebula with his three-feet mirror. Marvellous was the complexity and splendour of the object revealed to him, but not the trace of a star could be seen.

The end was not yet, however. Encouraged by the success of the three-feet telescope, Lord Rosse commenced the construction of one four times as powerful. After long and persistent labours, and at a cost, it is said, of thirty thousand pounds, the great Parsonstown reflector, with its wonderful six-feet speculum, was directed to the survey of the heavens. At Christmas 1845, while the instrument was yet incomplete, and in unfavourable weather, the giant tube was turned towards the Orion nebula. Professor Nichol was the first who saw the mysterious object as pictured by the great mirror. Although the observation was not successful so far as the resolution of the nebula was concerned, yet Nichol's graphic account of the telescope's performance is well worth reading:

'Strongly attracted in youth by the lofty conceptions of Herschel [he writes], I may be apt to surround the incident I have to narrate with feelings in so far of a personal origin and interest: but, unless I greatly deceive myself, there are few who would view it otherwise than I. With an anxiety natural and profound, the scientific world watched the examination of Orion by the six-feet mirror; for the result had either to confirm Herschel's hypothesis, in so far as human insight ever could confirm it; or unfold among the stellar groups a variety of constitution not indicated by those in the neighbourhood of our galaxy. Although Lord Rosse warned me that the circumstances of the moment would not permit me to regard the decision then given as absolutely final, I went in breathless interest to the inspection. Not yet the veriest trace of a star! Unintelligible as ever, *there* the nebula lay: but how gorgeous its brighter parts! How countless those streamers branching from it on every side! How strange, especially that large horn on the north, rising in relief from the black skies like a vast cumulous cloud! It was thus still possible that the nebula was irresolvable by human art; and so doubt remained. *Why* the concurrence of every favourable condition is requisite for success in such inquiries may be readily comprehended. The object in view is to discern, *singly*, sparkling points, small as the point of a needle, and close as the particles of a handful of sand; so that it needs but the smallest unsteadiness in the air, or imperfection in the shape of the reflecting surface, to scatter the light of each point, to merge them into each other, and present them as one mass.'

Before long Lord Rosse, after regrinding the great mirror, obtained better views of the mysterious nebula. Even now, however, he could use but half the power of the telescope, yet at length the doubts of astronomers as to the resolvability of the nebula were removed. 'We could plainly see,' he wrote to Professor Nichol, 'that all about the trapezium was a mass of stars, the rest of the nebula also abounding with stars, and exhibiting the characteristics of resolvability strongly marked.' These views were abundantly confirmed by subsequent observations with the great mirror.

It will surprise many to learn that even Lord Rosse's great reflector is surpassed in certain respects by some of the exquisite refractors now constructed by opticians. As a light-gatherer the mirror is, of course, unapproachable by refractors. Even if it were possible to construct an achromatic lens six feet in diameter, yet, to prevent flexure, a thickness would have to be given to the glass which would render it almost impervious to light and therefore utterly useless. But refractors have a power of definition not possessed by large reflectors. With a refractor eight inches only in aperture, for instance, Mr. Dawes has obtained better views of the planets (and specially of Mars) than Lord Rosse's six-feet speculum would give under the most favourable circumstances. And in like manner, the performance of Lord Rosse's telescope on the Orion nebula has been surpassed—so far as resolution into discrete stars is concerned—by the exquisite defining power of the noble refractor of Harvard College (U.S.). By means of this instrument hundreds of stars have been counted within the confines of the once intractable nebula.

It seemed, therefore, that all doubt had vanished from the subject which had so long perplexed astronomers. 'That was proved to be *real*,' Nichol wrote, 'which, with conceptions of space enlarged even as Herschel's, we deemed *incomprehensible*.'

Yet even at this stage of the inquiry there were found minds bold enough to question whether a perfectly satisfactory solution of the great problem had really been attained. Nor is it difficult, I think, to point out strong reasons for such doubts. I shall content myself by naming one which has always appeared to me irresistible.

The Orion nebula as seen in powerful telescopes covers a large extent of the celestial sphere. According to the Padre Secchi, who observed it with the great Merz refractor of the observatory at Rome, the nebulous region covers a triangular space, the width of whose base is some eight times, while its height is more than ten times as great as the moon's apparent diameter—a space more than fifty times greater than that covered by the moon. Now, I do not say that it is inconceivable that an outlying star-system, so far off as to be irresolvable by any but the most powerful telescopes, should cover so large a space on the heavens. On the contrary, I do not believe that a galaxy resembling our own would be resolvable at all, unless it were so near as to appear much larger than the Orion nebula. I believe astronomers have been wholly mistaken in considering any of the nebulæ to be such systems as our own. There may be millions of such systems in space, but I am very certain no telescope we could make would suffice to resolve any of them. But what I do consider inconceivable is, that a nebula extending so widely, and placed (as supposed) beyond our system, should yet appear to cling (as the Orion nebula undoubtedly does) around the fixed stars seen in the same field with it. So strongly marked is this characteristic, that Sir John Herschel (who failed, apparently, to see its meaning) mentions amongst others no less than four stars—one of which is the bright middle star of the belt—as 'involved in *strong* nebulosity,' while the intermediate nebulosity is only just traceable. The probability that this arrangement is accidental is so small as to be almost evanescent.

However, as I have said, English astronomers, almost without a dissentient voice, accepted the resolution of the nebula as a proof that it represents a distant star-system resembling our own galactic system, but far surpassing it in magnitude.

The time came, however, when a new instrument, more telling even than the telescope, was to be directed upon the Orion nebula, and with very startling results.

The spectroscope had revealed much respecting the constitution of the fixed stars. We had learned that they are suns resembling our own. It remained only to show that the Orion nebula consists of similar suns, in order to establish beyond all possibility of doubt the theories which had been so complacently accepted. A very different result rewarded the attempt, however. When Dr. Huggins turned his spectroscope towards the great nebula, he saw, in place of a spectrum resembling the suns, *three bright lines only!* A spectrum of this sort indicates that the source of light is a *luminous gas*, so that whatever the Orion nebula may be, it is most certainly *not* a congeries of suns resembling our own.

It would be unwise to theorise at present on a result so remarkable. Nor can we assert that Herschel's *speculations* have been confirmed, though his general reasoning has been abundantly justified. Astronomers have yet to do much before they can interpret the mysterious entity which adorns Orion's sword. On every side, however, observations are being made which give promise of the solution of this and kindred difficulties. We have seen the light of comets analysed by the same powerful instrument; and we learn that the light from the tail and coma is similar in quality (in indicating gasity, though by no means indicating the same gases) to that emitted from the Orion nebula. We see, therefore, that in our own solar system we have analogues of what has been revealed in external space. We know that many comets belong to the solar system, and it may be safely alleged that there is now not a particle of evidence that the nebula lies beyond our galaxy.

We need not doubt, however, the accuracy of Lord Rosse's observations. More than a year before his death, indeed, he mentioned to Dr. Huggins 'that the *matter* of the great nebula in Orion had not been resolved by his telescope. In some parts of the nebula he observed a large number of exceedingly minute *red* stars. These red stars, however, though apparently *connected* with the irresolvable material of the nebula, yet seemed to be distinct from it.'

The whole subject seems to be as perplexing as any that has ever been submitted to astronomers. Time, however, will doubtless unravel the thread of the mystery. We may safely leave the inquiry in the hands of the able observers and physicists whose attention has been for a long time directed towards it. And we need only note, in conclusion, that in the southern hemisphere there exists an object equally mysterious—the great nebula round η Argus—which has yielded similar results when tested with the spectroscope. The examination of this mysterious nebula, associated with the most remarkable variable in the heavens—a star which at one time shines but as a fifth magnitude star, and at another exceeds even the brilliant Canopus in splendour—may, for aught that is known, throw a new light on the constitution of the great Orion nebula.

From *Fraser's Magazine* for February 1869.

THE SUN'S TRUE ATMOSPHERE.

So much attention was directed to the solar corona during the discussions which preceded and followed the eclipse of 1870, that a discovery of extreme importance—but not at all associated with the corona—received far less attention than it deserved. The discovery I refer to was, in fact, more important in its bearing on problems of solar physics than any which have been made since Kirchhoff first told us how to interpret the solar spectrum. It was also intimately connected with the labours of that eminent physicist. I propose briefly to describe the nature of the discovery, and then to discuss some of the results to which it seems to point.

Astronomers have long seen reason to believe that the sun has an atmosphere. And by the word atmosphere I mean something more than mere vaporous or gaseous masses, such as the prominences have been shown to be. A solar envelope, complete and continuous as our own atmosphere, seems undoubtedly suggested by the appearance which the sun's image presents when thrown on a suitably prepared screen in a darkened room; for then the disc is seen to be shaded off continuously towards the edge, where its brilliancy is scarcely half as great as at the centre. The phenomenon is so readily seen, and so unmistakable, that it is with a sense of wonder one hears that Arago called it in question. To use the words of Sir John Herschel, 'the fact is so palpable that it is a matter of some astonishment that it could ever fail to strike the most superficial observer.' And, again, not only the light but the heat of the outer portions of the sun's image has been estimated. In this case we do not depend upon the perhaps fallible evidence of the eye, but on that of heat-measuring instruments. Fr. Secchi, measuring the heat of different parts of the solar image, has found that of the part near the centre nearly double that from the borders. Lastly, photography gives unmistakable evidence on the subject.

Now, when Kirchhoff discovered the meaning of the solar spectrum, it seemed clear to him that he had determined the nature and constitution of the solar atmosphere. Let us consider the nature of Kirchhoff's discovery.

He found that the dark lines across the rainbow-tinted streak forming the background (as it were) of the solar spectrum are due to the action of absorbing vapours. The vapours necessarily lie *outside* the source of that part of the sun's light which produces the rainbow-tinted streak. If those vapours could be removed for a while, we should see a simple rainbow-riband of light. Or if the vapours could be so heated as to be no less hot than the matter beneath them which produces the rainbow spectrum, they would no longer cause any dark lines to appear; but being cooler, and so giving out less light than they intercept, they cut out the dark spaces corresponding to their special absorptive powers. To use Mr. Lockyer's striking, though perhaps not strictly poetical description of their action, these vapours 'gobble up the light on its way to the observer, so that it comes out with a balance on the wrong side of the account.' Each vapour produces its own special set of lines, occupying precisely those parts of the spectrum which the vapour's light would illuminate if the vapour shone alone. For these vapours, notwithstanding their action in intercepting or absorbing portions of the sunlight, are themselves in reality glowing with a light so intense that the human eye could not bear to rest upon it. If we could examine the vapours we supposed just now removed from the sun, we should obtain the very lines of light which are wanting in the spectrum of the sun.

When Kirchhoff had recognised in this way the presence of absorptive vapours around the real light-globe of the sun, he judged that they form the solar atmosphere. Because, although his mode of observation was not such as to assure him that these vapours completely envelop the sun, yet the telescopic aspect of the sun, and especially that darkening near the edge to which I have just referred, seemed to leave room for no other conclusion. But at this stage of the inquiry Kirchhoff fell into a mistake. He judged that the solar corona was the atmosphere which produced the solar dark lines, as well as the darkening of the sun's disc near the edge. The mistake is one which, as it seems to me, he would have avoided had he taken into account the enormous pressure at which an atmosphere so extensive as the corona would necessarily exist under the influence of the sun's mighty attractive energies. It may easily be shown that if the outer parts of the corona were as rare as the contents of our so-called vacuum-tubes, or even a thousand times rarer, yet according to the laws which regulate atmospheric pressure, the density even at vast heights above the sun's surface would attain to many hundred times that of our heaviest gases. The pressure would, indeed, be so great that we can see no way of escaping the conclusion that, despite the enormous heat, the gases composing the imagined atmosphere would be liquefied or even solidified.

When the observers of the Indian eclipse of 1868 found that the coloured prominences are masses of glowing hydrogen with other gases intermixed, and when the prominence-spectrum was found to show the hydrogen lines as these appear when hydrogen exists at very moderate pressures, Kirchhoff's view had to be abandoned as altogether untenable. Wherever the vapours exist which produce the solar dark lines, they are undoubtedly not to be looked for in the corona.

But *there* the lines are. The absorptive action is exerted somewhere. The question is—*Where* are the absorptive vapours?

At this stage of the inquiry a very strange view was expressed by Mr. Lockyer—a view which appears to have been founded on a slight misapprehension of the principles of spectrum analysis. He put forward the theory that the absorptive action takes place below the level of the sun's surface as we see it.

But observations made by Fr. Secchi at Rome pointed to a view so different from Mr. Lockyer's, as to lead to a controversy which filled many pages of the *Comptes Rendus*, of the *Philosophical Magazine*, and of other publications—a controversy conducted, as too many philosophical discussions have been, with a somewhat unphilosophical acrimony.

Fr. Secchi had noticed that when the very edge of the sun's disc is examined with the spectroscope, the dark lines disappear from the spectrum, which thus becomes a simple rainbow-tinted streak. He judged, accordingly, that the absorbing atmosphere exists above the sun's real surface; for he believed that just at the edge the bright lines corresponding to the light from the vapours themselves so nearly equal in intensity the light of the solar spectrum, that no signs of difference can be detected; or, in other words, that the dark lines are obliterated. On the other hand, the glowing atmosphere cannot, he argued, reach much above the sun's surface, since otherwise the spectroscope would show the bright lines belonging to that atmosphere's light. Now, no such lines are visible. So far as the spectroscopic evidence is concerned, it would appear as though immediately above the sun's surface, as we see it, there came the *sierra*—that low range of prominence-matter which, strangely enough, some have regarded as an atmospheric envelope. The spectrum of the sierra shows beyond all question that, like the prominences, this region consists of glowing hydrogen, mixed up with a few, and at times with several other gases, but certainly not capable of accounting for the thousands of dark lines in the solar spectrum. It seems quite clear, also, that the sierra is not of the nature of an envelope at all.

Over the narrow layer which Secchi supposed to exist between the sun's surface and the coloured sierra, began, and presently waxed warm, the controversy above referred to. Fr. Secchi was positive that he could see the narrow continuous spectrum on which he founded his view; Mr. Lockyer was equally positive that the worthy father could see nothing of the kind. Fr. Secchi urged that his telescope was better than Mr. Lockyer's, and that he worked in a better atmosphere; Mr. Lockyer retorted that his spectroscope was better than Fr. Secchi's, and that the imagined superiority of the Roman atmosphere was a myth. Something was said, too, by the London observer about a large speculum which was to decide the question, though this mirror does not seem to have been actually brought into action. Both the disputants expressed full confidence that time would prove the justice of their several views.

Soon after, an observation was made by Mr. Lockyer which seemed to prove the justice of Fr. Secchi's opinion; for, on a very favourable day for observations, Mr. Lockyer was able to detect, *not* the narrow rainbow-tinted spectrum seen by Secchi, but a narrow strip of spectrum belonging to the region just outside the sun's edge, which showed hundreds of bright lines. Here seemed to be conclusive evidence of that shallow atmosphere of glowing vapours in which Fr. Secchi had faith. But Mr. Lockyer interpreted his observations differently. The presence of these vapours on this particular occasion he regarded as wholly exceptional, and the cause of the exception he held to be the energetic injection of vapours from beneath the surface of the sun.

At about this stage of the controversy I had occasion to consider the problems associated with the physical condition of the sun and his surroundings; and although I took no part in the discussion between Fr. Secchi and Mr. Lockyer, I expressed (in papers which I wrote upon the subject) opinions which agreed with the views of the Italian astronomer. It is necessary for me to present in this place my own reasoning on the question at issue, because it not only serves to introduce the special observation made last December, by which the problem has been finally solved, but also presents certain considerations which must be attended to in interpreting that observation.

In the first place, I noted that the darkening of the sun's disc near the edge, or rather the marked nature of that darkening, instead of showing (as had been so often stated) that the sun had a very deep atmosphere, proves on the contrary that his atmosphere must be exceedingly shallow by comparison with the dimensions of his globe. It is easy to show why this is: and although the considerations on which the matter depends are exceedingly simple, yet the case is by no means the first in which exceedingly simple considerations have been lost sight of by students of science. Suppose we have a brightly-white globe encased symmetrically within a globe of some imperfectly transparent substance—as green glass. Now, if the white globe is an inch in diameter and the green glass globe a yard in diameter, the brightness of the white globe will be more or less impaired according to the transparency of the glass; *but* it will not be much more impaired at the edge of the inner globe's disc than near the middle. For clearly, when we look at the middle, we look through a foot and a half of glass (wanting only half an inch), and when we look at the edge of the inner globe's disc, we also look through a foot and a half of glass (wanting only a small fraction of an inch). Neither the half inch in the one case, nor the small fraction of an inch in the other, can make any appreciable difference; so that the enclosing globe of glass cuts off as much light when we look at the centre of the inner globe's disc as when we look at the edge. But now suppose that the enclosing globe forms a mere shell around the inner one. Suppose, for instance, that the inner globe is a yard in diameter and the shell of glass only half an inch thick. Then in this case, as in the former, the brightness of the inner globe will be more or less impaired according to the transparency of the glass; but it will no longer be affected equally whether we look at the middle or at the edge of the inner globe's disc. In the former case we only look through half an inch of glass, in the latter we look through a much greater range of glass; as the reader will see at once if he draw two concentric circles nearly equal in size to represent the inner globe and its enclosing shell. It is easy to calculate how long the range of glass actually is in the latter case. I have just gone through the calculation, and find that when the eye is directed to the edge of the enclosed globe, its line of sight passes through rather more than four inches and a quarter, so that more than eight times as much light is absorbed as in the case where the eye looks at the middle of the inner globe's disc, or directly through half an inch of glass.

Now we cannot tell what proportion holds in the case of the sun's disc, because we do not know how much light has been absorbed where we look at the middle of the disc. All we know is that whatever remains *after* such absorption is about twice as much as we receive from near the edge of the disc. It is easily seen that this knowledge is insufficient for our requirements. But there can be no question whatever that the total absorption near the edge exceeds many times that near the middle of the disc; and on very reasonable assumptions as to this excess, it may readily be shown that the absorbing atmosphere cannot exceed some five or six hundred miles in depth. Probably it is even shallower.

Now, there is a circumstance which perfectly accounts for the non-recognition by spectroscopists of an atmosphere relatively so shallow as this. Let it be remembered, in passing, that the average height of the sierra may be set at about five thousand miles; so that the atmosphere we are dealing with would be at the outside but one-fifth as high as that fine rim of red light with saw-like edge which astronomers detected around the eclipsed sun in the total eclipses of 1842, 1851, and 1860. Still it might be thought that patience only would be needed to detect the signs of such an atmosphere, shallow though it be. But there is a peculiarity of telescopic observation which renders the recognition of such an atmosphere, if of less than a certain depth, not difficult merely, but impossible. It may be well to exhibit the nature of the peculiarity at length, because it is of considerable interest to all who possess or use telescopes. I take an illustrative case which seems, at first, to have little connection with my subject.

Every reader of this work has heard of the double stars, and I dare say most of those who read this particular article have seen many of these beautiful objects. It is known that some double stars are much closer than others, and we commonly hear it mentioned as a proof of the excellence of a telescope that it will divide such and such a double star. But it might seem that if a telescope of a certain size were constructed with extreme care, it should be capable of dividing *any* double star; because we might use an eye-piece of any magnifying power we pleased, and so, as it were, *force* apart the two star-images formed by the object-glass.

Instead of this being the case, however, there is a limit for every object-glass beyond which no separation is possible; for this reason, simply, that the star-images formed by the object-glass are not points of light, as they would be if they correctly represented the stars of which they are the optical images. The larger the object-glass (assumed to be perfect in construction) the smaller is the star-image;[1] but it has always a definite size, and if this size is such that the two images of the stars forming a pair actually touch or overlap, we cannot separate them by using highly magnifying eye-pieces.

Now, what is true of a star is true of every point of any object we examine with a telescope. The image of the point is always a circle of light, which, though minute, has not appreciable dimensions. The image of the object is made up of all these circles, which necessarily overlap. Nor let the reader suppose that on this account telescopic observation is untrustworthy. Precisely the same peculiarity affects ordinary vision. There is no such thing as a perfect optical image of an object; though neither eyesight nor telescopic vision need be regarded as deceptive on this account. Our power of seeing minute details is *limited* by this peculiarity, but we are not actually *deceived*. If microscopic writing be shown us, for instance, we may find ourselves, after poring over it for some time, unable to make out its meaning, the letters seeming all blended together; but we know what our failure really means, and do not fall into the mistake of concluding that there are no details because the actual details are inscrutable.

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[1] A curious illustration of this is given by the fact that a certain astronomer of old, having reduced the aperture of his telescope to a mere pin-hole, announced that he was thus enabled to measure the real globes of the stars, for, instead of seeing the stars through his telescope as minute points of light, he now saw them with discs like the planets. He thought he was improving the defining qualities of his telescope, instead of altogether destroying them.

Let us apply this consideration to the sun, and more particularly to the appearance presented by the edge of the sun's disc. The image of every point of this edge is a small circle; the combination of all these small circles must produce a ring of light all round the true outline of the disc. If the sun's atmosphere did not reach beyond this ring, then no contrivance whatever could render the atmosphere discernible, let the telescope be ever so perfect and the observer ever so clear-sighted or skilful. Now, the actual extension of this ring will be greater or less according as the object-glass of the telescope is less or greater. It may readily be shown that neither Mr. Lockyer's telescope nor Fr. Secchi's could *possibly* show any signs of a solar atmosphere under two hundred miles in depth, while in all probability an atmosphere four or five times as deep would escape their scrutiny.

Are we then to remain altogether in ignorance of such an atmosphere, supposing that it actually exists, and that the dark lines in the solar spectrum are due to its absorptive power? Is there no way of obviating the difficulty which has just been dealt with?

So far as the method of observing the sun when uneclipsed is concerned, the answer to these questions must be negative; or, rather, it must be answered that our only hope of meeting the difficulty consists in increasing the size of the telescopes with which the sun is spectroscopically studied. And inasmuch as Dr. Huggins is preparing to apply the powers of a much larger telescope than either Mr. Lockyer's or Fr. Secchi's, we may possibly still hope to hear that the relatively shallow atmosphere can be studied when the sun is not eclipsed. For we may now speak of the existence of this atmosphere as a demonstrated fact. The difficulty which seemed to present insuperable obstacles to the observers who study the uneclipsed sun has been overcome by the ingenuity of one of the most skilful of those very observers—Professor Young, of America—when studying the solar eclipse of last December.

If, during any total eclipse of the sun, the moon *just* concealed the whole of the sun's disc (as may well happen), and if our satellite were only complaisant enough to stay still for a few minutes in such a position, so that one of these exact total eclipses could be studied as readily as one of greater extent (which never can happen), then the shallow atmosphere I have been speaking of could be recognised. The difficulty above considered would no longer exist. For the ring of light which actually hides the shallow atmosphere when the sun is not eclipsed, is an extension of the bright rim of the disc outwards: if the disc is completely hidden, there is no bright rim to be extended, and anything existing close by the sun's globe can be recognised.

But then, unfortunately, no total eclipse can present these desirable features. If a total eclipse is to be worth seeing at all, the moon's disc as seen at the time must be appreciably larger than the sun's. When totality begins the outlines of the two discs just touch at a single point, and when totality ends the two discs just touch at another point; but during all the rest of the totality the two outlines do not touch at all, that of the moon surrounding without touching that of the sun. The outlines of the two discs do twice touch, however, in each case for one moment and at one point. What Professor Young determined to do, therefore, was to bring under special examination that one point where the outlines touch at the exact moment when totality begins. In other words, he directed his special attention to the point where the last trace of the sun's disc was about to disappear. It is perhaps scarcely necessary to say that he did not trust to the powers of his telescope, but that he employed a powerful spectroscope. And further, he did not depend on his own observations alone, but had adjusted a spectroscope for the use of Mr. Pye, an English gentleman residing in the part of Spain where the eclipse-observing parties were stationed, so that that gentleman also might make the required observations.

In his account, Professor Young does not mention what he expected to see. It is probable that he had in his thoughts the observations of Fr. Secchi, and hoped to obtain evidence respecting that shallow atmospheric envelope which Secchi believed in and Lockyer rejected; though it is quite possible he merely desired to ascertain whether the constitution of the lower part of the sierra differed in any marked respect from that of the upper portion. As the moment approached when the last fine sickle of sunlight was to be obscured, the solar spectrum which was visible in the spectroscopic field of view grew rapidly fainter. The region actually examined by Professor Young was in reality a narrow, almost linear space, touching the edge of the sun's disc; so that before totality had commenced he had the light from our own illuminated atmosphere, and not direct sunlight, to deal with. Thus he had just such a solar spectrum as is seen when a spectroscope is directed to the sky in the daytime. But as the moment of totality drew near, the illumination of the atmosphere, and with it the brightness of the rainbow-tinted streak, rapidly diminished. At last the solar spectrum vanished; and then—*What* was it replaced by? What was found to be the spectrum of the solar atmosphere close by the sun's surface? In place of the rainbow-tinted riband crossed by thousands and thousands of dark lines, there appeared a new and most beautiful spectrum—a riband of *rainbow-tinted lines*, thousands in number and of all degrees of thickness,—hundreds of red lines, and then, in order, hundreds of orange lines, hundreds of yellow, green, indigo, and violet lines, like coloured cross-threads on a black riband, only infinitely more beautiful. A charming spectacle, truly, but so short-lived that no man can ever hope, though he lived to four-score years and ten, to let his eyes rest in all his life for more than ten or twelve seconds on the beautiful array of coloured lines which two men only have as yet beheld. We may increase the dimensions and power of our telescopes until the existence of these lines can be recognised without the aid of eclipse-darkness, but the lines can never be seen, save during eclipse, as Young and his colleague saw them in December 1870. And these observers tell us that in a second or two the lines vanished, the advancing moon hiding the shallow solar atmosphere. If it should ever be given to any man to see six total eclipses (which has never yet happened to any), and to successfully apply in each instance the method employed by Professor Young, then in all, during his life, that man would have seen the beautiful line-spectrum to perfection for some ten or twelve seconds; but not otherwise can even so long a total period of observation be secured. No single observer, then, can hope to learn much about the thousands of lines which have still to be mapped during eclipse opportunities.

But now let us consider the import of the observation. What are these myriads of coloured lines? Every dark line of the solar spectrum, says Professor Young, seemed to have its representative in this bright-line spectrum. Many of the groups of lines which had flashed so quickly into view and endured but so brief a period were familiar to him; in other words, his study of the solar spectrum had made him conversant with the corresponding groups of dark lines. It follows, then, beyond all possibility of question, that the source of light was a highly complex atmosphere, formed of those very vapours which, by their absorptive power, produce the dark lines—formed, that is, of the vapours of iron and of copper, of zinc, sodium, magnesium, and of all those elements whose presence in the sun's substance had been inferred from the study of the solar spectrum.

Here, then, at length we have the true solar atmosphere, an atmosphere of a highly complex nature, and doubtless exceedingly dense near the visible surface of the sun, because subject to a pressure so enormous. The upper limit of this atmosphere cannot lie very far above the sun's surface, at least not very far compared with the sun's dimensions. Supposing the actual time during which the line-spectrum was visible to have been two seconds, then it is easy to tell how deep the atmosphere is. For in two seconds the moon must have traversed a space corresponding to about three hundred miles at the sun's distance. An atmosphere three hundred miles deep is, therefore, indicated by Professor Young's observations. It need hardly be said, however, that in the excitement of eclipse observation, the estimate of minute intervals of time can scarcely be relied upon, unless checked by instrumental arrangements, which was not the case in the present instance. We may fairly conclude that the depth of the solar atmosphere lies between some such limits as a hundred miles and five hundred miles.

In the above estimate, I have supposed the measurement to be made from the sun's visible surface. But it is very unlikely that that surface is the true lower limit of the atmosphere. It seems far more probable that the surface we see is merely a layer of clouds (as Sir William Herschel suggested so long ago) in the solar atmosphere, and that the actual depth of the atmosphere is more truly indicated by the appearances seen when large sun-spots are examined. That these spots are cavities has been abundantly established. That they are openings through layers of solar clouds has not been indeed demonstrated, yet it is difficult to conceive how they can otherwise be interpreted. As to the way in which the spots are formed, theorists are at issue, some urging that there is an uprush from depths beneath the solar surface; others, that there is a downrush of matter from without. But neither of these views is in any way incompatible with Herschel's theory, that the spots are openings in solar cloud-layers.

We might thus be led to compare the solar atmosphere with our own, though it will of course be obvious that there are many marked points of difference. But in our own atmosphere we have at least two distinct cloud-levels, the region, namely, where the *cumulus* or wool-pack clouds are formed, and that where the *cirrus* or feathery clouds make their appearance. There is air above the cirrus clouds, air between the cirrus and cumulus layers, and air between the cumulus clouds and the earth. And precisely in the same way we may conceive that there exists at all times a solar atmospheric region beneath as well as above the cloud-layer which forms the sun's visible surface, and beneath and between the other cloud-layers revealed by telescopic observations.

But passing from the very difficult question suggested by the consideration of regions *below* the sun's visible surface, let us discuss briefly the bearing of Professor Young's discovery upon our views respecting those outer regions—the coloured prominences and sierra, the corona itself, and, in fine, all the portions of space which lie above the true atmosphere.

In the first place, it seems to me that the discovery disposes finally of the theory that the coloured *sierra* is an atmospheric envelope, properly so-called. I had long since been led to question whether the sierra could be so regarded. Let me remind the reader that the sierra is nothing more nor less than the region which Lockyer rediscovered in 1868. It had, in fact, been recognised by telescopists since 1806, the name *sierra* having been given to it by the observers of the eclipse of 1842. It is a red region, having (as its name implies) a serrated upper surface, as seen in the telescope, and seemingly extending all round the sun's disc. The red prominences appear to spring from its upper surface. Strangely enough, when Lockyer made his ingenious observations of the coloured prominences, he had not heard of this discovery, or had forgotten it. Accordingly, finding traces of prominence-matter all round the sun, he concluded that there was a continuous envelope of hydrogen (mixed with some other gases) surrounding the whole of the sun's globe. It was probably through being misled by this supposition that he gave to the sierra a new name—entitling it the *chromosphere*—announcing at the same time that its upper surface was smooth in outline. Respighi, the eminent Italian spectroscopist—also working, it would seem, in ignorance or forgetfulness of the prior recognition of the layer—announced presently that the upper surface of the so-called chromosphere[1] was altogether irregular—more irregular, in fact, than the surface of a tempest-tossed sea. On re-examining the sierra, Mr. Lockyer found this to be the case. But perhaps the most striking evidence as to the real aspect of the sierra was afforded during the eclipse of last December, when Fr. Secchi, towards the close of totality, saw around the western half of the moon's disc a complete semicircle of sierra, and noted that this beautiful coloured crescent was formed of multitudes of minute prominences. This agrees very satisfactorily with my own anticipatory description of the probable nature of the sierra, when I suggested that the sun's surface is probably 'covered at all times with small prominences, bearing somewhat the same relation to the gigantic "horns" and "boomerangs" seen during eclipses that the bushes covering certain forest regions bear to the trees.'

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[1] It affords strange evidence of the caution with which new names should be suggested, that this name, embodying, as we see, an erroneous theory, and also perpetuating the remembrance of a mistaken claim, is scarcely yet beginning to fall into disuse. Perhaps its Greek origin and its length may have something to do with this; for although astronomy—at least descriptive astronomy—has hitherto not been disfigured by the hideous nomenclature which botanists and geologists seem to rejoice in, yet there is always a large class of science students who delight in sesquipedal names, as giving an air of profundity to their discourse. It may even be dangerous to hint that the true form of the compound for a *colour-sphere* is not *chromo-sphere*, but *chromato-sphere*, since the extra syllable will multiply tenfold the favour with which the compound is accepted. When will tyros learn that the true lover of science

'Projicit ampullas et sesquipedalia verba ?'

But the larger prominences have been shown by Zöllner and Respighi to be phenomena of eruption. They are masses of glowing gas, which have been flung from great depths beneath the visible surface of the sun. May we not conclude that the smaller prominences which constitute the sierra are of like nature? that they also have been flung from beneath the sun's visible surface? As respects the larger prominences we can have no manner of doubt, because they have been *seen* to be flung out in eruptive sort. And this refers to all orders of prominences, except only those very numerous and relatively very small prominences which crowd together so as to form the seemingly continuous coloured sierra. These cannot be watched as the others have been. But it seems highly probable that those among them which are not the remains of loftier prominences are, like their larger fellows, phenomena of eruption.

Again, as respects the corona, all the evidence we have is opposed to the conception that the phenomenon is atmospheric. It shows two regions, which, though not separated by well-defined limits from each other, may yet be regarded as, in a sense, distinct. There is an inner and brighter portion, which the sesquipedalians have proposed to call the *leucosphere*,—apparently on the *lucus a non lucendo* principle, for it is neither white nor spherical. And there is the outer portion, much less brilliant, and much more strikingly radiated. Neither one part nor the other presents a single feature suggestive of an atmospheric nature;[1] and the certainty that the two portions belong to a single object affords yet more conclusive evidence against this interpretation of the corona. But the rays of the corona are of a somewhat remarkable nature. When well seen, as during the eclipse of 1868, they are pointed; and even during so unfavourable an eclipse as that of December last, the dark spaces between the rays are seen to widen rapidly with increased distance from the sun. These pointed radiations serve to show that coronal rays must be, in reality, shaped somewhat as cones, having their bases towards the sun. The idea is startling enough, but, admitting the accuracy of the pictures made during well-seen eclipses, and of the Astronomer-Royal's account of the corona during the eclipses of 1851 and 1860, there is no escape from the conclusion here stated. It is not more certain that the sun is a globe, and not a flat disc as he seems to be, than that the coronal radiations are not flat pointed rays, but cone-shaped. Yet no one will suppose that there are a number of monstrous cone-shaped masses

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[1] I am here referring to the possibility that the corona may be due to some species of solar atmosphere. The theory that the corona is due to light in our own atmosphere has now at length been definitely abandoned by all astronomers.

—atmospheric or otherwise—standing, as it were, upon the sun's surface. I can see no other way of accounting for these conical extensions than by regarding them as phenomena indicating some form of repulsive action exerted by the sun.

But whatever opinion we may form on this and kindred problems, it seems clear that we must regard the envelope discovered by Professor Young as the only true solar atmosphere: and a very strange and complex atmosphere it is. Nothing yet learned respecting the sun's surroundings surpasses in interest this fiery envelope, in which some of the most familiar of our metals appear as glowing vapours. If anything could add to the interest attaching to the coloured prominences and sierra, it is the fact now revealed that they are propelled through this wonderful envelope, over which they float for a while with strangely changing figure. Truly the study of solar physics, which twenty years ago seemed at a stand-still, is advancing with rapid strides; and it seems scarcely possible to exaggerate the interest either of what has been already revealed, or of the discoveries which are likely to be effected during the approaching eclipse.

From the *St. Paul's Magazine* for May 1871.

ADDENDUM.—Doubts were urged, for some time after this paper appeared, as to the reality of Young's discovery. But during the total eclipse of December 1871, and yet again during the annular eclipse of 1872, decisive evidence was obtained in its favour, and it is now received by all.

SOMETHING WRONG WITH THE SUN.

When we consider the intense heat which has prevailed in Europe during July 1872, and the circumstance that in America also the heat has been excessive, insomuch that in New York the number of deaths during the weak ending July 6 was three times greater than the average, we are naturally led to the conclusion that the sun himself is giving out more heat than usual. Though not endorsing such an opinion, which, indeed, is not warranted by the facts, since terrestrial causes are quite sufficient to explain the recent unusual heats, we cannot refrain from noting, as at least a curious coincidence, that at the very time when the heat has been so great, the great central luminary of the solar system has been the scene of a very remarkable disturbance—an event, in fact, altogether unlike any which astronomers have hitherto observed.

Certain Italian spectroscopists—Respighi, Secchi, Tacchini, and others—have set themselves the task of keeping a continual watch upon the chromatosphere. They draw pictures of it, and of the mighty coloured prominences which are from time to time upreared out of, or through, the chromatospheric envelope. They note the vapours which are present, as well as what can be learned of the heat at which these vapours exist, their pressure, their rate of motion, and other like circumstances. It was while engaged in some of the more difficult and delicate of these tasks that Tacchini noticed the strange occurrence now to be described.

'I have observed a phenomenon,' he says, 'which is altogether new in the whole series of my observations. Since May 6, I had found certain regions in the sun remarkable for the presence of magnesium.' Some of these extended half-way round the sun. This state of things continued, the extension of these magnesium regions gradually growing greater, until at length, 'on June 18,' says Tacchini, 'I was able to recognise the presence of magnesium quite round the sun—that is to say, the chromatosphere was completely invaded by the vapour of this metal. This ebullition was accompanied by an absence of the coloured prominences, while, on the contrary, the flames of the chromatosphere were very marked and brilliant. It seemed to me as though I could see the surface of our great source of light renewing itself.' While this was going on Tacchini noticed (as had frequently happened before in his experience) that the bright streaks on the sun which are called faculæ were particularly brilliant close to those parts of the edge of the disc where the flames of the chromatosphere were most splendid and characteristic. The granulations also, which the astronomer can recognise all over the sun, when a large telescope is employed, were unusually distinct.

Tacchini concludes (and the inference seems just) that there had not been a number of local eruptions of magnesium vapour, but complete expulsions. Only we would venture to substitute for the word 'expulsion' the expression 'outflow' or 'uprising,' since it may well be that these vapours rise by a quiet process resembling evaporation, and not by any action so violent that it could properly be regarded as expulsive.

In whatever way, however, the glowing vapour of magnesium thus streamed into the envelope of the sun, it would seem that the aspect of our luminary was modified by the process—not indeed in a very striking manner, or our observers in England would have noticed the change, yet appreciably. 'More than one person,' says Tacchini, 'has told me that the light of the sun has not at present its ordinary aspect; and at the Observatory we have judged that we might make the same remark. The change must be attributed to magnesium.'

It is impossible to consider attentively the remarkable occurrence recorded by Tacchini without being struck by the evidence which it affords of solar mutability. We know that during thousands of years our sun has poured forth his light and heat upon the worlds which circle around him, and that there has been no marked intermittence of the supply. We hear, indeed, of occasions when the sun has been darkened for a while; and we have abundant reasons for believing that he has at times been so spot-covered that there has been a notable diminution of the supply of light and heat for several days together. Yet we have had no reasons for anticipating that our sun might permanently lose so much of his heat and lustre that the inhabitants of earth would suffer. Tacchini's observation reminds us, however, that processes are at work upon the sun which admit of being checked or increased, interrupted altogether or exaggerated so violently, that the whole aspect of the sun, his condition as the fire and lamp of the planetary system, may be seriously affected.

If we only remember that our sun is one of the stars, not in any way distinguished, unless perhaps by relative insignificance, from the greater number of the stars which illuminate our skies at night or are revealed by the telescope, we shall learn to recognise the possibility that he may undergo marked changes. There are stars which after shining with apparent steadiness for thousands of years (possibly for millions of years before astronomy was thought of), have become suddenly much reduced in brightness, or after a few flickerings (as it were) have gone out altogether. There are others which have shone with equal steadiness, and have then suddenly blazed out for a while with a lustre exceeding a hundredfold that which they formerly possessed. It would be equally unpleasant for ourselves whether the sun suddenly lost the best part of his light, and presently went out altogether, or whether he suddenly grew fiftyfold brighter and hotter than he now is. Yet in the present position of sidereal astronomy it is quite impossible to assert confidently that one event or the other might not take place at any time.

Fortunately, we may view this matter (just as astronomers have learned to view the prospect of mischievous collisions with comets) as a question of probabilities. Among so many thousands of stars there have been so many sudden outbursts of light and fire, so many sudden defalcations of splendour. Our sun is one of those thousands, and so far as we know takes his chance with the rest.

From the *Spectator* for August 1872.

THE SUN'S SURROUNDINGS.

Observations during the eclipse of July 1878, considered in connection with recently acquired knowledge respecting meteor systems, open before us a new and very interesting page of the book of nature. It remains still to be read, and many years will be required for its thorough study, but even now we can recognise the general character of its contents. We refer to the recognition by Professors Newcomb and Langley of an extension of faint luminosity to a distance of about 6 deg. on either side of the sun. Other observers also recognised either as great an extension of coronal luminosity or such peculiarities of shape in the luminous region around the sun as show that under slightly more favourable conditions they would have seen all that Newcomb and Langley saw.

It may be well now, when the attention of astronomers has been directed afresh to meteoric systems,

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to re-examine the important series of observations made during the last eclipse.

Hitherto in eclipse observations no luminous appearances have been satisfactorily recognised at a greater distance from the sun than perhaps about two of his own diameters, or some one and a quarter million of miles. Unquestionably rays have been traced to a greater distance which could not possibly be regarded as other than solar appendages, including under that name all phenomena, whether coronal, zodiacal, cometic, or meteoric, which have relation to the sun, as distinguished from luminous appearances having their origin in regions not farther away than the moon. Any long ray extending from the eclipsed sun and visible in a constant position during totality must of necessity be due to a solar appendage. Such rays have been indeed referred to matter nearer than the moon; and as our own air most certainly could not explain them (for not a particle of our own air within 6 or 7 deg. of the eclipsed sun is in sunlight at mid-totality), scattered particles of matter travelling between the earth and the moon have been imagined in explanation of these long rays. An ingenious experiment was devised in illustration of this theory. If an irregular plug be inserted into a round hole (or a round plug into an irregular hole) in the shutter of a darkened room in such sort that the sun's light enters the room through the interstices, appearances very much resembling these long rays will be seen where the solar rays fall on the dust-laden air of the room. In this experiment the plug represents the moon, the sun is himself (as Launce would have said), and the air in the room represents the cis-lunar matter of the theory. But those who have adopted the theory and accepted this ingenious illustration appear not to have observed a trifling distinction between the circumstances of an eclipse and those of the suggested illustration. The shutter is a rather necessary adjunct to the experiment. If the sun's rays only pass in through small interstices to illuminate the air in the room, the radiating streaks are observed; but if they fall freely into the room (save where a small opaque disc is suspended in the window to represent the moon), no such rays can be seen. Now in the eclipse the plug is represented well enough by the moon, but the shutter is omitted. The omission is somewhat important. To say truth, no one (certainly no mathematician) who takes fully into account the actual circumstances of a total eclipse of the sun can fail to perceive that a long ray seen in an unchanged position during the whole continuance of total eclipse must certainly belong to regions far beyond the body of the moon. Now since such rays have been seen on several occasions, we have in one sense satisfactory evidence of the existence of solar appendages outside the corona.

But it has been held that until such rays have been seen and identified by at least two observers on the same occasion, they must not be treated as scientific realities, however certain the individual observers of such features may have been that their descriptions

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were exact. It had so happened that evidence of this kind had not hitherto been obtained. Different observers had seen long rays on the same occasion, but their accounts differed widely. Often, indeed, it seemed impossible to believe that the same rays had been observed. From what we have since learned, however, respecting the corona itself, we perceive that in the excitement of an eclipse observers are capable of the wildest errors of description and delineation. Until 'the retina which never forgets' had viewed the corona, it was objected that an object which different observers saw under entirely different aspects could not possibly be a real solar appendage; but photography has shown that the discrepancies were due to inexact observation. They were found to have no existence in the corona itself as photographed from widely separated stations. If photographs of the long rays could have been obtained, the discrepancies in their case would doubtless have been removed in like manner. But, unfortunately, the lustre of these long rays is exceedingly faint, and hitherto they have not been shown in any photograph of an eclipse. Hereafter greater success may be obtained in this direction, possibly by doing what was strongly urged long before the late eclipse—devoting, namely, the whole duration of totality to obtain one really effective photograph. There is no longer the slightest advantage to be obtained from taking several photographs, for the doubt which formerly rendered that course necessary (the suspicion that the corona changes during totality) is no longer entertained. For the present, however, we have only drawings or descriptions to deal with. It is the distinctive feature of the observations made in July 1878, that these drawings and descriptions show most satisfactory accordance. We propose to consider a few of them, taking first those which give to the rays or luminous projections their least extension.

In the first place, we take a drawing by Mr. J. N. Lockyer. In this drawing the black body of the moon is shown surrounded by a narrow ring of light, the inner corona. Outside the ring are three projections, nearly in the ecliptic. (It is important to notice this, for the axis of the zodiacal light is at all times nearly in the ecliptic.) On the eastern side there is one projection, shaped like a long isosceles triangle, the base of which is on the moon's edge. On the western side are two nearly equal projections, bearing somewhat the same relation to the single eastern one that the two sides of the tail of a wind-vane bear to the pointed head. The three projections are not very unequal in length, the longest extending about 1¼ diameter, or about two-thirds of a degree, from the sun's edge. This would correspond to rather more than 1,000,000 miles. The resemblance to a wind-vane, says Mr. Lockyer, 'was almost perfect, (the corona) being pointed at one end, and bounded by parallel edges at the other:' 'others,' he adds, 'saw a resemblance to a fish's tail.' The breadth of the wind-vane in his picture is about three-fourths of the sun's diameter. It is worthy of notice that in the telescope the streamers vanished utterly. 'Not a shred of them was left.' This shows the extreme faintness of their light: the slight absorption of light by the glass of the telescope (a 3¾-inch Cook) sufficed entirely to obliterate these delicate solar appendages.

General Myer, the head of the Army Signal Service, and commonly known in America as 'Old Probabilities,' because the weather forecasts daily published appear under that heading (without the adjective), saw the corona from the summit of Pike's Peak, 14,200 feet above the sea level. He described the corona as showing five radial lines of a golden colour, beyond which in the direction of the ecliptic 'were prolonged bright silver rays.' This was seen only with the naked eye. 'In the telescope the appearance was quite different; a layer close to the sun only, of a light pink colour, was seen; the long bright silver rays had disappeared.' It is worthy of notice that General Myer had on a former occasion seen the long coronal rays in circumstances almost as favourable. He watched the progress of the eclipse of 1869 from the summit of White Top Mountain, near Abingdon, Virginia, 5,530 feet above the sea level. On that occasion he saw in the telescope only an aureola of clear, yellowish, bright light, closely surrounding the moon's disc, and fading gradually into the tint of the darkened sky, 'with a slight tinge of pinkish green' (a colour almost as difficult to imagine as the 'gris rouge' mentioned in Molière's *L'Avare*). But to the unaided eye the eclipse presented 'a vision magnificent beyond description.' Around the full and intensely black disc of the moon there was an aureola of a soft bright light, 'through which shot out as if from the circumference of the moon straight, massive, silvery rays, seeming distinct and separate from each other, to a distance of two or three diameters of the lunar disc, the whole spectacle showing as upon a background of diffused rose-coloured light.' It is specially interesting to note the arrangement of the long rays on that occasion. For the eclipse of 1869 occurred on August 7, the time of central eclipse for the whole earth being 9h. 46m. P.M. Greenwich time; and the eclipse of 1878 happened on July 29, the time of central eclipse being 9h. 23m. P.M. Greenwich time; so that the two eclipses occurred at the same time of the year within nine days 23 minutes. The actual directions in which the observers of the two eclipses looked at the sun were inclined to each other in an angle of less than nine degrees. So that, in point of fact, we may say that very nearly the same view was obtained of the sun's surroundings on both occasions, apart from any changes which may have occurred in the interval. If it should appear that the long rays presented the same general aspect in 1869 as they did in July 1878, then the inference would be that they are objects not changing as the sierra, the prominences, and the inner corona change. We could not safely conclude that this is the case; but it would be the most probable inference. Now, General Myer, in 1869, said that the silvery rays were longest and most prominent at four points of the circumference, two upon the upper and two upon the lower portion, apparently equidistant from each other, giving the spectacle a quadrilateral shape. The angles of the quadrangle were about opposite the north-eastern, north-western, south-eastern, and south-western points of the disc. The description is not exact, but it accords well with the conclusion that two of the rays (the north-eastern and south-western) were in the ecliptic, and the other two at right angles to the ecliptic. If so, there seems good reason, as will now appear, for believing that the rays seen by Myer in 1869 may be the same, or, rather belong to the same cosmical appendage of the sun, as those seen by him and by other observers in July 1878.

We take next a brief description of the rays as seen by Mr. Alfred C. Thomas, from Capitol Hill, near Denver City. 'In the plane of the ecliptic,' he says, 'the streamers of light extended for about 1½ times the diameter of the moon.' Three accounts, then—Mr. Lockyer's, General Myer's, and Mr. Thomas's—agree in describing the extension of the rays in the direction of the ecliptic. We might quote other accounts, for, in fact, many observers saw these ecliptic streamers. They were seen by so many as to leave no doubt of their reality. We have now, however, to consider the evidence of those who saw these ecliptic rays to a much greater distance, and of one observer who saw also rays at right angles to them which other observers failed to recognise, but which must not on that account be regarded as subjective phenomena.

Professor Cleveland Abbe had intended to observe the eclipse from Pike's Peak, but he was taken ill a short time after reaching the summit, and had to be removed to the Lake House (hotel) on the day preceding the eclipse. Here, without instruments and too ill to sit up, he made his observations. He could do nothing but observe the corona with the naked eye; but as he gave his whole attention during totality to the coronal rays, his observations are very valuable. By Monday, the day of the eclipse, he had recovered sufficiently to be laid on the ground upon a gentle slope facing westward. He says:—'I was undisturbed by any other consideration, except to get a true presentation of the rays, which I had hitherto firmly believed to be either in the earth's atmosphere or in the observer's eyes. I went over the region around the sun again and again at least six times leisurely during the 161 seconds of totality, and cannot doubt the truthfulness and fairness of my drawing and description.'

He saw the streamers which other observers had compared to a wind-vane; but he traced them to a much greater distance than other observers. The point of the vane, as he saw it, reached to a distance from the sun equal to fully six diameters. (A weekly contemporary says six degrees, but Professor Abbe's description and picture, both which we have before us as we write, agree in making the distance six diameters.) This would correspond to more than five million miles. On the other side, the double streamer forming the tail of the vane does not extend (in the picture) nearly so far, not more than three or four diameters, or about three million miles. The breadth of the vane where it crosses the sun is almost exactly equal to the sun's diameter, so that the sides of the vane just touch the sun's edge. Athwart the wind-vane streamers, at right angles, and forming a very similar system, lies another set of streamers, a pointed one above the sun, corresponding to the pointed eastern end of the wind-vane set, and a broad, rather fan-shaped streamer below the sun corresponding to the broad western end or tail of the wind-vane. The pointed ray extends fully five diameters from the sun. Its light was fainter, which accounts for its having escaped the attention of other observers with less time at their disposal to study the corona with the naked eye. It is very important, especially in connection with the choice of methods for observing future eclipses, to note Professor Abbe's experience on this point. He says:—'All the details came out only after repeated examinations of the corona and repeated attempts to draw and note its peculiarities. I am satisfied that a glance of a few minutes will no more suffice to do justice to these delicate phenomena than it could to enable a naturalist to draw the distinguishing features of a new shell or insect, or would enable an artist to correctly sketch in a landscape.'

We can, then, understand why other observers, even some who recognised a yet greater extension of the coronal light, failed to perceive the fainter thwart system of rays seen by Professor Abbe. We also perceive that these thwart rays cannot possibly be rejected as merely subjective phenomena. They were seen by Abbe as satisfactorily, and remained as persistently visible, as the ecliptical rays which so many saw; but, being fainter, they were not detected by those who (like all except Abbe) gave only a short portion of totality to the study of the corona with the naked eye.

Only Professors Langley and Newcomb saw the coronal light extending further than the long rays observed by Professor Abbe. Langley, an astronomer to whom we owe the finest drawing of a large sun-spot ever yet seen, was at the head of a party from Pittsburg. Like Myer, he observed the eclipse from the summit of Pike's Peak. We do not know whether he adopted any special method of observing the faint outlying part of the coronal luminosity; but he traced it in the most transparent atmosphere to a distance of twelve diameters of the sun on one side, and three on the other. Its extension was in the direction of the ecliptic, and the light resembled the zodiacal. In other words, what Langley saw at a distance of about twelve diameters from the sun was a faint and softly graduated luminosity, not the separate rays seen to a distance of about six diameters only. Newcomb saw a similar luminosity, and traced it to the same distance from the sun. Like Langley, also, Newcomb seems not to have specially observed the long rays seen by those who examined attentively the regions closer to the sun. As Newcomb used a screen which concealed these regions from view, during at least part of the time devoted to observations with the naked eye, we need not be surprised that he failed to see features which Cleveland Abbe, studying these regions alone during the whole of totality, found sufficiently delicate to require his whole attention.

From a comparison of all the observations, the following important conclusions seem established beyond all possibility of doubt or question:—Outside the solar sierra, averaging some 6,000 or 7,000 miles in height, comes the prominence region, extending about 100,000 miles from the sun's surface. Outside this comes the inner corona, shining in part with its own light, sometimes coming chiefly from multitudes of solid or liquid bodies in a state of incandescence, sometimes chiefly from glowing vaporous matter. This region extends from 200,000 to 500,000 miles from the sun. Beyond the inner corona is the outer corona, as already known and photographed during the eclipses of 1870 and 1871, and extending about 1,000,000 miles from the sun. But far outside the outer corona there is a region occupied by matter so situated and so illuminated (or possibly self-luminous) as to present the appearance of long rays extending, if we may judge from observations hitherto made, directly from the sun to a distance of 5,000,000 miles. Outside this region again lies another in which, whether by the combination of multitudes of such rays as are seen separately close to the sun, or through the presence of matter in other forms, a softened luminosity prevails which during total eclipse can be traced along the zodiac at least 10,000,000 miles from the surface of the sun. Lastly, from observations made during evening twilight in spring, and during morning twilight in autumn (at which twilight hours the zodiac near the sun is most nearly upright during the year), we can trace the extension of the zodiacal luminosity seen by Langley and Newcomb to distances exceeding seven or eight times at least those to which they traced it during total eclipse. Nay, there are reasons for believing that at times this luminosity has been traced to such a distance from the sun as to show that the zodiacal matter extends much further from him than the orbit of our own earth.

Now, in one sense, the relations here presented are not new. The zodiacal light has been known from the time of Childrey, if not from that of Tycho Brahe. Mathematicians have long seen that it must belong to a solar appendage, rejecting utterly the doctrine advanced by some that it comes from matter travelling round our own earth. Again, the long coronal rays had been very confidently regarded by most mathematical astronomers, and indeed by all who had sufficiently studied the evidence, as belonging to matter near the sun. And though the zodiacal had never before been recognised during totality, and so the gap between the outermost coronal rays and the innermost part of the zodiacal seen during twilight had never been observationally filled up, yet the mind's eye of science had clearly discerned even that portion of the zodiacal. Still the recognition of the whole range of solar surroundings, in such sort that no question can any longer, it should seem, be raised as to their reality, even by those least able to follow scientific reasoning, cannot but be regarded as an important step. Many will now study eclipse phenomena with a new interest and a new purpose, who formerly supposed the theories of astronomers respecting the unseen parts of the zodiacal to be mere hypotheses, even if they were not wholly fanciful speculations. Many in like manner will study the zodiacal light as seen in morning and evening twilight with much greater care than heretofore. Its changes of extent, position, and lustre, will now be seen to be full of interest. Whether they synchronise or not with the changes undergone by the corona, or with the varying extent and activity of the prominence region, or with the number and size of the solar spots, are questions of importance, to some of which we may hope to obtain an answer, seeing that in tropical regions, especially at elevated stations, the zodiacal (as Humboldt long since pointed out) is a conspicuous phenomenon. Whether during future eclipses the zodiacal will be traced further from the sun than by Langley and Newcomb in July 1878, is not in reality a matter of much moment. The great point is that the zodiacal should once have been unmistakably recognised during total eclipse. That was all that was wanted to make the chain of evidence complete even for those who cannot recognise the force of reasoning when it educes from observation something more than was actually seen. Now that this is done, eclipse observations of the zodiacal will not be wanted. They never could throw any light on the nature of the zodiacal light, except near the sun, where the long rays are. For at a distance of 6 deg. or 7 deg. from the eclipsed sun the light of our own atmosphere begins to show, obliterating the delicate light of sun-surrounding matter. As to the rays, now that their solar, or rather cosmical, nature is recognised, we may expect that they will be carefully studied. What their real nature may be is not easy to determine. That they indicate the existence of meteoric or cometic systems near the sun, as Professor Abbe believes, may be accepted as probable. Indeed, the only reasonable theory of the zodiacal is one which, as I showed nine years ago, would lead us to expect to see signs of meteoric streams where these rays appear. I wrote (*Treatise of the Sun*, 1st edition, p. 363):—'The sun has as an appendage a cloud of cosmical bodies, which will continue for ever, or for an indefinitely long period, as a cloud appendage. It will not be fixed, the relations even of its several parts will not be fixed; on the contrary, the cloud will shift and fluctuate, its members aggregating here and segregating there; but as a clustering appendage it will be permanent. . . . The existence of multitudes of eccentric systems implies necessarily the aggregation of meteors in the sun's neighbourhood. . . . These meteors would be severally illuminated with inconceivable splendour on account of their nearness to the sun. . . . Add to this that those approaching most nearly to him would be rendered incandescent if not vapourised by the intensity of his heat, and that most probably electric discharges would take place between them on account of the intense energy of the solar action.'

But Professor Abbe's theory, that the rays he saw were themselves meteor systems, involves difficulties which he does not seem to have noticed. A meteor system would only by the merest chance appear as a beam lying directly athwart the sun. For that purpose it must lie beyond the sun. (Professor Abbe says it may lie either on the hither or further side of the sun, but a stream of meteors on the hither side would rapidly fade out near the sun, just as Mercury and Venus rapidly lose their lustre when approaching the sun in the part of their orbit nearest to the earth, or technically when approaching inferior conjunction.) Again, such a system to appear straight athwart the sun must lie in a plane passing through the earth's surface at the time. Now among some two hundred meteor systems through which our earth passes, we find every variety of position. So that it might happen that a single meteor seen during totality would have just such a position as would make it appear to lie athwart the sun. Even that would be a strange chance. That two systems (two only being seen, be it remembered) should be thus situated would be a very strange chance indeed. But that in every eclipse ever observed the meteor systems should be so situated as to form rays apparently extending from the sun, is altogether incredible. Sometimes most assuredly, if Abbe's theory were correct, we should see meteor systems passing near to the sun without touching his disc. But beams of silvery light so situated have never been seen. There are other objections, but this may suffice here. We seem compelled to believe that the extension of the rays directly from the sun is not an accidental feature, but is due to the real extension of lines of illuminating matter radially from his globe. The explanation of the peculiarity remains to be discovered. I venture to predict that it will be related, and not remotely, to the explanation of the extension of comets' tails directly from the sun; for it is more than probable that enormous quantities of cometic matter exist in the sun's immediate neighbourhood.

I would quote in conclusion another passage from the work to which I referred a few lines above, because, though written nine years ago, it corresponds well with the present position of the subject I have been dealing with :—

'To doubt what general view we should form of the corona and zodiacal light seems to savour, not of that wise caution which prevents the true philosopher from overlooking difficulties, but rather of an inaptitude to estimate the value of evidence. As to details, we may be doubtful. Other matter than meteoric or cometic matter may well be in question; other modes of producing light, save heat, electricity, or direct illumination, may be in operation in this case; and, lastly, there may be other forces at work than the attractive influence of solar gravity, or the form of repulsive force indicated by the phenomena of comets. As regards also the true shape and position of the coronal and zodiacal appendage, and yet more as regards its variations in shape, we may still have much to learn. But of the general fact that the corona and zodiacal light form a solar appendage of amazing extent and importance, that they are not merely terrestrial phenomena, but worthy of all the attention astronomers and physicists can direct to them, no reasonable doubts can any longer be entertained.'

From the *Times*, Sept 6, 1879.

NEWS FROM HERSCHEL'S PLANET.

SATURN—the *altissimus planeta* of the ancients—remains still the most distant planet respecting whose physical condition astronomers can obtain satisfactory information. The most powerful telescopes yet constructed have been turned in vain towards those two mighty orbs which circle outside the path of distant Saturn: from beyond the vast depths which separate us from Uranus and Neptune, telescopists can obtain little intelligence respecting the physical habitudes of either planet. Nor need we be surprised at the failure of astronomers, when we consider the difficulties under which the inquiry has been conducted. In comparing the telescopic aspect of Uranus with that of Saturn (for example) we must remember that Uranus is not only twice as far from the earth but also twice as far from the sun as Saturn is. So that the features of Uranus are not merely reduced in seeming dimensions, in the proportion of about one to four, but they are less brilliantly illuminated in the same proportion. And therefore (roughly) any given portion of the surface of Uranus—say a hundred miles square near the middle of his visible disc—sends to us but about one-sixteenth part of the light which an equal and similarly-placed portion of the surface of Saturn would send to us. Now every astronomer knows how difficult it is, even with very powerful telescopes, to study the physical features of Saturn. A telescope of moderate power will show us his ring-system and some of his satellites; but to study the belts which mark his surface, the aspect of his polar regions, and in particular those delicate tints which characterise various portions of his disc, requires a telescope of great power. It will be understood, therefore, that in the case of Uranus, which receives so much less light from the sun, and is so much farther from us, even the best telescopes yet made by man must fail to reveal any features of interest. We may add also that Uranus is a much smaller planet than Saturn, though far larger than the combined volume of all the four planets, Mars, Venus, the Earth, and Mercury. If Saturn (without his rings) and Uranus were both visible together in the same telescopic field (a circumstance which may from time to time happen) the Herschelian planet would appear so small and faint that it might readily be taken for one of Saturn's moons, the ringed planet sending us altogether some sixty times as much light as Uranus.

But what the telescope had hitherto failed to accomplish, has been achieved by means of that wonderful ally of the telescope, the spectroscope, in the able hands of the eminent astronomer and physicist, Dr. Huggins. News has been received about the constitution of the atmosphere of Uranus, and news so strange (apart from the strangeness of the mere fact that any information could be gained at all respecting a vaporous envelope so far away) as to lead us to speculate somewhat curiously respecting the conditions under which the Uranians, if there are any, have their being.

Before describing the results of Dr. Huggins's late study of the planet, it may be well to give a brief account of what is known respecting Uranus.

The question has been raised whether Uranus was known to the astronomers of old times. There is nothing altogether improbable in the supposition that in countries where the skies are unusually clear, the planet might have been detected by its motions. Even in our latitude Uranus can be quite readily seen on clear and moonless nights, when favourably situated. He shines at such times as a star of about the fifth magnitude—that is, somewhat more brightly than the faintest stars visible to the naked eye. In the clear skies of more southerly latitudes he would appear a sufficiently conspicuous object, though, of course, it would be wholly impossible for even the most keen-sighted observer to recognise any difference between the aspect of the planet and that of a star of equal brightness. The steadiness of the light of Saturn causes this planet to present a very marked contrast with the first magnitude stars whose lustre nearly equals his own. But although the stars of the lower orders of magnitude scintillate like the leading orbs, their scintillations are not equally distinguishable by the unaided eye. Nor is it unlikely that if Uranus were carefully watched (without telescopic aid) he would appear to scintillate slightly. Uranus would only be recognisable as a planet by his movements. There seems little reason for doubting, however, that even the motions of so faint a star might have been recognised by some of the ancient astronomers, whose chief occupation consisted in the actual study of the star groups. We might thus understand the Burmese tradition that there are eight planets, the sun, the moon, Mercury, Venus, Jupiter, and Saturn, and another named Ráhu which is invisible. If Uranus was actually discovered by ancient astronomers, it seems far from unlikely that the planet was only discovered to be lost again, and perhaps within a very short time. For if anything positive had been learned respecting the revolution of this distant orb, the same tradition which recorded discovery of the planet would probably have recorded the nature of its apparent motions.

Be this as it may, we need by no means accept the opinion of Buchanan, that if the Burmese tradition relates to Uranus, Sir William Herschel must be 'stripped of his honours.' The rediscovery of a lost planet, especially of one which had remained concealed for so many centuries, must be regarded as at least as interesting as the discovery of a planet altogether unknown. Nor was there any circumstance in the actual discovery of Uranus, which would lose its interest, even though we accepted quite certainly the conclusion that the Herschelian planet was no other than old Ráhu.[1]

Let us turn to Herschel's own narrative of his detection of Uranus. It is in many respects very instructive.

In the first place, we must note the nature of the work he was engaged upon. He had conceived the idea of measuring the distances of the stars, or at least of the nearer stars, by noting whether as the earth circles around the sun the relative positions of stars lying very close to each other seemed to vary in any degree. To this end he was searching the heavens for those objects which we now call double stars, most of which were in his day supposed to be not in reality pairs of stars—that is, not physically associated together—but seen near together only because lying nearly in the same direction. The brighter star of a pair was in fact supposed to lie very much nearer than the fainter; and it was because, being so much nearer, the brighter star should be much more affected (seemingly) by the earth's motion around the sun, that Herschel hoped to learn much by studying the aspect of these unequal double stars at different seasons of the year. He hoped yet more from the study of such bright orbs as are surrounded by several very faint stars. It was a case of this kind that he was dealing with, when accident led him to the discovery of Uranus. 'On Tuesday, the 13th of March (1781),' he writes, 'between ten and eleven in the evening, while I was examining the small stars in the neighbourhood of Eta in Gemini, I perceived one that appeared visibly larger than the rest. Being struck with its uncommon magnitude, I compared it to Eta and the small stars in the quartile between Auriga and Gemini, and finding it so much larger than either of them, suspected it to be a comet. I was then engaged in a series of observations (which I hope soon to have the opportunity of laying before the Royal Society) requiring very high powers, and I had ready at hand the several magnifiers of 227, 660, 932, 1,536, 2,010, &c., all of which I successfully used on that occasion. The power I had on when I first saw the (supposed) comet was 227. From experience I knew that the diameters of the fixed stars are not proportionally magnified with higher powers, as those of the planets are; therefore I now put on the powers of

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[1] It is, after all, at least as likely that Ráhu—assuming there really was a planet known under this name—might have been Vesta, the brightest of the small planets which circle between Mars and Jupiter, as the distant and slow-moving Uranus. For although Vesta is not nearly so bright as Uranus, shining, indeed, only as a star of the seventh magnitude, yet she can at times be seen without telescopic aid by persons of extremely good sight; and her movements are far more rapid than those of Uranus. In the high tablelands of those eastern countries, where some place the birth of astronomy, keen-sighted observers might quite readily have discovered her planetary nature, whereas the slow movements of Uranus would probably have escaped their notice.

660 and 932, and found the diameter of the comet increased in proportion to the power, as it ought to be on a supposition of its not being a fixed star, while the diameters of the stars to which I compared it were not increased in the same ratio. Moreover, the comet being magnified much beyond what its light would admit of, appeared hazy and ill-defined with these great powers, while the stars presented that lustre and distinctness which from many thousand observations I knew they would retain. The sequel has shown that my surmises were well founded.'

There are three points to be specially noted in this account. First, the astronomer was engaged in a process of systematic survey of the celestial depths—so that the discovery of the new orb cannot be properly regarded as accidental, although Herschel was not at the time on the look-out for as yet unknown planets. Secondly, the instruments he was employing were of his own construction and device, and probably no other in existence in his day would have led him to the discovery that the strange orb was not a fixed star. And thirdly, without the experience he had acquired in the study of the heavens, he would not have been able to apply the test which, as we have seen, he found so decisive. The fact that the stars are not magnified by increased telescopic power to the same extent as planets or comets, is, as Professor Pritchard has justly remarked, 'an important result of the undulatory theory of light, and was unsuspected in Sir William Herschel's day.' So that whether we consider the work Herschel was engaged upon, the instruments he used or the experience he had acquired, we recognise the fact that he alone of the astronomers of his time was capable of discovering Uranus otherwise than by a fortunate accident. Others might have lighted on the discovery—indeed, we shall presently see that the real wonder is that Uranus had not been for many years a recognised member of the solar system—but no one except Herschel could within a few minutes of his first view of the planet have pronounced confidently that the strange orb (whatever it might be) was not a fixed star.

I do not propose to enter here, at length, into the series of researches by which it was finally demonstrated that the newly-discovered body was not a comet but a planet, travelling on a nearly circular path around the sun, at about twice Saturn's distance from that orb. With this part of the work Herschel had very little to do. To use Professor Pritchard's words, having ascertained the apparent size, position, and motion of the stranger, 'Herschel very properly consigned it to the care of those professional astronomers who possessed fixed instruments of precision in properly constituted observatories—to Dr. Maskelyne, for instance, who was then the Astronomer-Royal at Greenwich, and to Lalande, who presided over the observatory in Paris.' As the newly-discovered body travelled onwards upon its apparent path, astronomers gradually acquired the means of determining what its real path might be. At first they were misled by erroneous measures of the stranger's apparent size, which suggested that the supposed comet had in the course of the first month after its discovery approached to within half its original distance. At length, setting aside all these measures, and considering only the movements of the stranger, Professor Saron was led to the belief that it was no comet, but a member of the solar system. It was eventually proved, chiefly by the labours of Lexell, Lalande, and the great mathematician Laplace, that this theory fully explained all the observed motions of the newly-discovered body, and before long (so complete is the mastery which the Newtonian system gives astronomers over the motions of the heavenly bodies) all the circumstances of the new planet's real motions became very accurately known. It was now possible, not only to predict the future movements of the stranger, but to calculate his motions during former years. This last process was quickly applied to the planet, with the object of determining whether among the records of observations made on stars, any might be detected which related in reality to the newly-discovered body. The result will appear at first sight somewhat surprising. The new planet had actually been observed no less than nineteen times before that night when Herschel first showed that it was not a fixed star, and those observations were made by astronomers no less eminent than Flamsteed, Bradley, Mayer, and Lemonnier. Flamsteed had seen the planet five several times, each time cataloguing it as a star of the sixth magnitude, so that five such stars had to be dismissed from Flamsteed's lists. But the case of Lemonnier was even more singular; for he had actually observed the planet no less than twelve times, several of his observations having been made within the space of a few weeks. 'M. Arago naturally comments,' says Professor Pritchard, 'on the want of system displayed by Lemonnier in 1769; had he but reduced and arranged his observations in a properly-constructed register, his name instead of Herschel's would have been attached for all time to one of the starry host. But Lemonnier was not a man of order; his astronomical papers are said to have been a very picture of chaos; and M. Bouvard, to whom we have long been indebted for the best tables of the new planet, narrates that he had seen one of Lemonnier's observations of this very star written on a paper bag which had contained hair powder!'

In our days, when fresh planets are being discovered and named in the course of each year that passes, it may appear strange that much difficulty was found in assigning a suitable name to the stranger. But we must remember that for ages the planetary system had been supposed to comprise no other primary members than those known to the ancients. The discovery of Uranus was an altogether novel and unlooked-for circumstance. It was not supposed that fresh discoveries of like nature would be made, still less that a planet would hereafter be discovered under circumstances far more interesting even than those which attended the discovery of Uranus. Accordingly a mighty work was made before Uranus was fitted with a name. Lalande proposed the name of the discoverer, and the new planet was indeed long known on the Continent by the name of Herschel. The symbol of the planet (♅), the initial letter of Herschel's name with a small globe attached to the cross-stroke, still reminds us of the honour which Continental astronomers generously proposed to render to their fellow-worker in England.[1] Lichtenberg proposed the name of Astræa, the goddess of justice—for this 'exquisite reason,' that since justice had failed to establish her reign upon earth, she might be supposed to have removed herself as far as possible from our unworthy planet. Poinsinet suggested that Cybele would be a suitable name; for since Saturn and Jupiter, to whom the gods owed their origin, had long held their seat in the heavens, it was time to find a place for Cybele, 'the great mother of the gods.' Had the supposed Greek representative of Cybele—Rhæa—been selected for the honour, the name of the planet would have approached somewhat nearly in sound, and perhaps in signification, to the old name Ráhu. But neither Astræa nor Cybele were regarded as of sufficient dignity and importance among the ancient deities to supply a name for the new planet.[2] Prosperin proposed Neptune as a suitable name, because Saturn would thus have the eldest of his sons on one side of him, and his second son on the other. Bode at length suggested the name of Uranus, the most ancient of the deities; and as Saturn, the father of Jupiter, travels on a wider orbit than Jupiter, it was judged fitting that an even wider orbit than Saturn's should be adjudged to Jupiter's grandfather. In accepting the name of Uranus for the new planet, astronomers seemed to assert a belief that no planet would be found to travel on a yet wider path; and accordingly when a more distant planet was discovered, the suggestion of Prosperin had to be reconsidered; but it was too late to change the accepted nomenclature, and accordingly the younger brother of Jupiter has had assigned to him a planet circling outside the paths of the planets assigned to their father and grandfather. It may be noted, also, that a more appropriate name for the new planet would have been Cœlus, since all the other planets have received the Latin names of the deities.

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[1] There is a certain incongruity, accordingly, among the symbols of the primary planets. Mercury is symbolised by his *caduceus*, Venus by her looking-glass (I suppose), Mars by his spear and shield, Jupiter by his throne, Saturn by his sickle; and again, when we pass to the symbols assigned to the planets discovered in the present century, we find Neptune symbolised by his trident, Vesta by her altar, Ceres by her sickle, Minerva by a sword, and Juno by a star-tipped sceptre. Uranus alone is represented by a symbol which has no relation to his position among the deities of mythology.

[2] Both these names are found among the asteroids, the fifth of these bodies (in order of discovery) being called Astræa, the eighty-ninth being named after the great mother of gods and goddesses.

Herschel himself proposed another name. As Galileo had called the satellites of Jupiter the Medicean planets, while French astronomers proposed to call the spots on the sun the Bourbonian stars, so Herschel, grateful for the kindness which he had received at the hands of George III., proposed that the new planet should be called Georgium Sidus. On account of the interest attaching to all Herschel's remarks respecting his discovery, I quote in full the letter in which he submitted this proposition to Sir Joseph Banks, then the President of the Royal Society. 'By the observations of the most eminent astronomers in Europe,' he remarks, 'it appears that the new star, which I had the honour of pointing out to them in March 1781, is a primary planet of our solar system. A body so nearly related to us by its similar condition and situation in the unbounded expanse of the starry heavens, must often be the subject of the conversation, not only of astronomers, but of every lover of science in general. This consideration, then, makes it necessary to give it a name, whereby it may be distinguished from the rest of the planets and fixed stars. In the fabulous ages of ancient times, the appellations of Mercury, Venus, Mars, Jupiter, and Saturn, were given to the planets, as being their principal heroes and divinities. In the present more philosophical era, it would be hardly allowable to have recourse to the same method, and call on Juno, Pallas, Apollo, or Minerva, for a name to our new planet. The first consideration in any particular event or remarkable incident seems to be its chronology; if in any future age it should be asked *when* this last-found planet was discovered, it would be very satisfactory to say, "In the reign of George III." As a philosopher, then, the name of Georgium Sidus presents itself to me as an appellation which will conveniently convey the information of the time and country where and when it was brought to view. But as a subject of the best of kings, who is the liberal protector of every art and science; as a native of the country from whence this illustrious family was called to the British throne; as a member of that society which flourishes by the distinguished liberality of its royal patron; and last of all as a person now more immediately under the protection of this excellent monarch and owing everything to his unlimited bounty, I cannot but wish to take this opportunity of expressing my gratitude by giving the name of Georgium Sidus—

> 'Georgium sidus
> —jam nunc assuesce vocari,'—

to a star which, with respect to us, first began to shine under his auspicious reign.' Herschel concludes by remarking that, by addressing this letter to the President of the Royal Society, he takes the most effectual method of communicating the proposed name to the *literati* of Europe, which he hopes 'they will receive with pleasure.'

Herschel's proposition found little favour, however, among Continental astronomers. Indeed it is somewhat singular that for some time two names came into general use—one in Great Britain and the other on the Continent—neither being the name eventually adopted for the planet. In books published in England for more than a quarter of a century after the discovery of Uranus we find the planet called either the Georgium Sidus or the Georgian. For a shorter season the planet was called on the Continent either the Herschelian planet or simply Herschel. Many years elapsed before the present usage was definitely established.

In considering Herschel's telescopic study of the planet, we must remember that, owing to the enormous length of time occupied by Uranus in circling round his orbit, the astronomer labours under a difficulty distinct in character from the difficulties which have already been considered. As Jupiter and Saturn circle on their wide orbits they exhibit to us—the former in the course of eleven years, the latter in the course of twenty-nine and a half years—all those varying presentations which correspond to the seasons of these planets. Jupiter, indeed, owing to the uprightness of his axis (with reference to his path), presents but slight changes. But Saturn's globe is at one time bowed towards us, so that a large portion of his north polar regions can be seen, and anon (fifteen years later) is so bowed, that a large portion of his southern polar regions can be seen; while between these epochs we see the globe of Saturn so posed that both poles are on the edge of his disc, and then only does the shape of his disc indicate truly the compression or polar flattening of the planet.

But although similar changes occur in the case of Uranus, they occupy no less than eighty-four years in running through their cycle, or forty-two years in completing a half-cycle—during which, necessarily, all possible presentations of the planet are exhibited. Now it is commonly recognised among telescopists that the observing time of an astronomer's life—that is, the period during which he retains not merely his full skill, but the energy necessary for difficult researches—continues but about twenty-five years at the outside.

So that few astronomers can hope to study Uranus in all his presentations, as they can study Mars, or Jupiter, or Saturn.

When we add to this circumstance the extreme faintness of Uranus, we cannot wonder that Herschel should have been unable to speak very confidently on many points of interest. His measures of the planet's globe were sufficiently satisfactory, and, combined with modern researches, show that Uranus has a diameter exceeding the earth's rather less than four and a half times. Thus the surface of Uranus exceeds that of our globe about twenty times, and his bulk is more than eighty times as great as the earth's. His volume, in fact, exceeds the combined volume of Mercury, Venus, the Earth, and Mars, almost exactly forty times. But Sir W. Herschel was unable to measure the disc of Uranus in such a way as to determine whether the planet is compressed in the same marked degree as Jupiter and Saturn. All that he felt competent to say was that the disc of the planet seemed to him to be oval, whether he used his seven-feet, or his ten-feet, or his twenty-feet reflector. Arago has expressed some surprise that Herschel should have been content with such a statement. But in reality the circumstance is in no way surprising. For as a matter of fact Herschel had been almost foiled by the difficulty of measuring even the planet's mean diameter. The discordance between his earliest measures is somewhat startling. His first estimate of the diameter made it ten thousand miles too small (its actual value being about thirty-four thousand miles); his next made it nearly three thousand miles too great; while his third made it ten thousand miles too great. His contemporaries were even less successful. Maskelyne, after a long and careful series of observations, assigned to the planet a diameter eight thousand miles too small; the astronomers of Milan gave the planet a diameter more than twenty thousand miles too great; and Mayer, of Mannheim, was even more unfortunate, for he assigned to the planet a diameter exceeding its actual diameter of thirty-four thousand miles, by rather more than fifty thousand miles. It will be understood, therefore, that Herschel might well leave unattempted the task of comparing the different diameters of the planet. This task required that he should estimate a quantity (the difference between the greatest and the least diameters) which was small even by comparison with the *errors* of his former measurements.

But besides this, a peculiarity in the axial pose of Uranus has to be taken into account. I have spoken of the uprightness of Jupiter's axis with reference to his path; and by this I have intended to indicate the fact that, if we regard Jupiter's path as a great level surface, and compare Jupiter to a gigantic top spinning upon that surface, this mighty top spins with a nearly upright axis. In the case of Uranus the state of things is altogether different. The axis of Uranus is so bowed down from uprightness as to be nearly in the level of the planet's path. The result of this is that when Uranus is in one part of his path, his northern pole is turned almost directly towards us. At such a time we should be able to detect no sign of polar flattening even though Uranus were shaped like a watch-case. At the opposite part the other pole is as directly turned towards the earth. Only at the parts of his path between these two can any signs of compression be expected to manifest themselves; and Uranus occupies these portions of his path only at intervals of forty-two years.

Herschel would have failed altogether in determining the pose of Uranus but for his discovery that the planet has moons. For the moons of the larger planets travel for the most part near the level of their planet's equator. We can, indeed, only infer this in the case of Uranus (for even the best modern measurements cannot be regarded as satisfactorily determining the figure of his globe), but the inference is tolerably safe.

For six years Herschel looked in vain for Uranian satellites. His largest telescopes, supplemented by his wonderful eyesight and his long practice in detecting minute points of light, failed to reveal any trace of such bodies. At length he devised a plan by which the light-gathering power of his telescopes was largely increased. On the 11th of January, 1787, he detected two satellites, though several days elapsed before he felt justified in announcing the discovery. At intervals during the years 1790–1798, he repeated his observations; and he supposed that he had discovered four other satellites. He expresses so much confidence as to the real existence of these four bodies, that it is very difficult for those who appreciate his skill to understand how he could have been deceived. But he admits that he was unable to watch any of these satellites during a considerable part of its path, or to identify any of them on different nights. All he felt sure about was that certain points of light were seen which did not remain stationary, as would have happened had they been fixed stars. No astronomer, however, has since seen any of these four additional satellites, though Mr. Lassell has discovered two which Herschel could not see (probably owing to their nearness to the body of the planet). As Mr. Lassell has employed a telescope more powerful than Herschel's largest reflector, and has given much attention to the subject, no one has a better right to speak authoritatively on the subject of the four additional satellites. Since, therefore, he is very confident that they have no existence, I feel bound to represent that view as the most probable; yet I am unable to pass from the subject without expressing a hope that one of these days new Uranian satellites will be revealed.

The four known moons travel backwards; that is, they circle in a direction opposed to that in which all the planets of the solar system, and all the moons of Jupiter and Saturn, as well as our own moon, are observed to travel. Much importance has been attached to this peculiarity: but in reality the paths of the Uranian moons are so strangely situated with respect to the path of Uranus, that the direction in which they travel can hardly be compared with the common direction of the planetary motions. Imagine the path of Uranus to be represented by a very large wooden hoop floating on a sheet of water; then, if a small wooden hoop were so weighted as to float almost upright, with one half out of the water, the position of that hoop would represent the position of the path of one of the planet's satellites. It will be seen at once that if we suppose a body to travel round (and upon) the former hoop in a certain direction, then a body travelling round the latter hoop could scarcely be said to travel in the same direction, whether it circled one way or the other. Or, to employ another illustration—if a watch be laid face upward on a table, we should correctly say that its hands move from east through south to west; but, if it be held nearly upright and the face rather upwards, we should scarcely say that the hands moved from east through *south* to west; nor if the face were tilted a little further forward, so as to be inclined rather downwards, should we say that the hands move from east through *north* to west.

The great slope or tilt of the paths is undoubtedly a more singular feature than the direction of motion. Implying as it does that the planet's globe is similarly tilted, it suggests the strangest conceptions as to the seasonal changes of the planet. It seems impossible to suppose that the inhabitants of Uranus, if there are any, can depend on the sun for their supply of heat· The vast distance of Uranus from the sun, although reducing the heat-supply to much less than the three-hundredth part of that which we receive, is yet an insignificant circumstance by comparison with the axial tilt. One can understand at least the *possibility* that some peculiarity in the atmosphere of the planet might serve to remedy the effects of the former circumstance; precisely as our English climate is tempered by the abundant moisture with which the air is ordinarily laden. But while we can conceive that the minute and almost star-like sun of the Uranian skies may supply much more heat than its mere dimensions would lead us to expect, it is difficult indeed to understand how the absence of that sun for years from the Uranian sky can be adequately compensated. Yet in Uranian latitudes corresponding to the latitude of London the sun remains below the horizon for about twenty-three of our years in succession. Such is the Arctic[1] night of regions in Uranus occupying a position corresponding to that of places in our temperate zone.

But the most important result of the discovery of the satellites has been the determination of the mass or weight of the planet, whence also the mean density of its substance has been ascertained. It has been thus discovered that, like Jupiter and Saturn, Uranus is constructed of much lighter materials than the earth. Our earth would outweigh almost exactly six times a globe as large as the earth but no denser than Uranus. It is to be noticed that in this respect the outer planets resemble the sun, whose density is but about one-fourth that of the earth. It seems impossible that the apparent size of any one of the outer planets can truly indicate the dimensions of its real globe. An atmosphere of enormous extent must needs surround, it would seem, the liquid or solid nucleus which probably exists within the orb we see.

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[1] It has been remarked that there is some incongruity in the name Arctic planets which I have assigned in my 'Other Worlds' to Uranus and Neptune, when considered with reference to the theory I have enunciated that these planets still retain an enormous amount of inherent heat. Many seem to imagine that the term arctic implies cold. I have, of course, only used the name as indicating the distance of Uranus and Neptune from the sun.

In the case of Jupiter or Saturn, the telescope has told us much which bears on this point; and as I have indicated in my 'Other Worlds,' and elsewhere, there is an overwhelming mass of evidence in favour of the theory that those orbs are still instinct with their primeval fires. But in the case of Uranus, it might well be deemed hopeless to pursue such inquiries, otherwise than by considering the analogy of the two larger planets. Direct evidence tending to show that the atmosphere of Uranus is in a condition wholly differing from that of our own atmosphere, cannot possibly be obtained by means of any telescopes yet constructed by men. Some astronomers assert that they have seen faint traces of belts across the disc of Uranus; but the traces must be very faint indeed, since the best telescopes of our day fail to show any marks whatever upon the planet's face. Even if such belts can be seen their changes of appearance cannot be studied systematically.

It is, however, on this very subject—the condition of the planet's atmosphere—that the discovery I have now to describe throws light.

Faint as is the light of Uranus, yet when a telescope of sufficient size is employed, the spectrum of the planet is seen as a faint rainbow-tinted streak. The peculiarities of this streak, if discernible, are the means whereby the spectroscopist is to ascertain what is the condition of the planet's atmosphere. Now, Father Secchi, studying Uranus with the fine eight-inch telescope of the Roman Observatory, was able to detect certain peculiarities in its spectrum, though it would now appear that (owing probably to the faintness of the light) he was deceived as to their exact nature. He says: 'The yellow part of the spectrum is wanting altogether. In the green and the blue there are two bands, very wide and very dark.' But he was unable to say what is the nature of the atmosphere of the planet, or to show how these peculiarities might be accounted for.

Recently, however, the Royal Society placed in the hands of Dr. Huggins a telescope much more powerful than either the Roman telescope or the instrument with which Dr. Huggins had made his celebrated observations on sun and planets, stars and star-cloudlets. It is fifteen inches in aperture, and has a light-gathering power fully three times as great as that possessed by either of the instruments just mentioned.

As seen by the aid of this fine telescope the spectrum of Uranus is found to be complete, 'no part being wanting, so far as the feebleness of its light permits it to be traced.' But there are six dark bands, or strong lines, indicating the absorptive action of the planet's atmosphere. One of these strong lines corresponds in position with one of the lines of hydrogen. Now it may seem at a first view that since the light of Uranus is reflected solar light, we might expect to find in the spectrum of Uranus the solar lines of hydrogen. But the line in question is too strong to be regarded as merely representing the corresponding line in the solar spectrum; indeed, Dr. Huggins distinctly mentions that 'the bands produced by planetary absorption are broad and strong in comparison with the solar lines. We must conclude, therefore, that there exists in the atmosphere of Uranus the gas hydrogen, sufficiently familiar to us as an element which appears in combination with others, but which we by no means recognise as a suitable constituent (at least to any great extent) of an atmosphere which living creatures are to breathe.[1] And not only must hydrogen be present in the atmosphere of Uranus, but in such enormous quantities as to be one of the chief atmospheric constituents. The strength of the hydrogen line cannot otherwise be accounted for. If by the action of tremendous heat all the oceans of our globe could be changed into their constituent elements, hydrogen and oxygen, it is probable that the signs by which an inhabitant of Venus or Mercury could recognise that such a change had taken place would be very much less marked than the signs by which Dr. Huggins has discovered that hydrogen exists in the atmosphere of Uranus. It will indeed be readily inferred that this must be the case, when the fact is noted that no signs whatever of the existence of nitrogen can be recognised in the spectrum of Uranus, though it is difficult to suppose that nitrogen is really wanting in the planet's atmosphere. Dr. Huggins also notes that none of the lines in the spectrum of Uranus appear to indicate the presence of carbonic acid. Nor are there any lines in the spectrum of Uranus corresponding to those which make their appearance in the solar spectrum when the sun is low down, and is therefore shining through the denser atmospheric strata. Most of these lines are due to the presence of aqueous vapour in our atmosphere, and it would seem to follow that if the vapour of water exists at all in the atmosphere of Uranus its quantity must be small compared with that of the free hydrogen.

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[1] Traces of hydrogen can nearly always be detected in the air,—but the quantity of hydrogen thus shown to be present is almost infinitesimally small compared with the amount of oxygen and nitrogen.

Admitting that the line seen by Dr. Huggins is really due to hydrogen—a fact of which he himself has very little doubt—we certainly have a strange discovery to deal with. If it be remembered that oxygen, the main supporter of such life as we are familiar with, cannot be mixed with hydrogen without the certainty that the first spark will cause an explosion (in which the whole of one or other of the gases will combine with a due portion of the other to produce water), it is difficult to resist the conclusion that oxygen must be absent from the atmosphere of Uranus. If hydrogen could be added in such quantities to our atmosphere as to be recognisable from a distant planet by spectroscopic analysis, then no terrestrial fires could be lighted, for a spark would produce a catastrophe in which all living things upon the earth, if not the solid earth itself, would be destroyed. A single flash of lightning would be competent to leave the earth but a huge cinder, even if its whole frame were not rent into a million fragments by the explosion which would ensue.

Under what strange conditions then must life exist in Uranus, if there be indeed life upon that distant orb! Either our life-sustaining element, oxygen, is wanting; or, if it exists in sufficient quantities (according to our notions) for the support of life, then there can be no fire, natural or artificial, on that giant planet. It seems more reasonable to conclude that, as had been suspected for other reasons, the planet is not at present in a condition which renders it a suitable abode for living creatures.

The *St. Paul's Magazine* for October 1871.

THE TWO COMETS OF THE YEAR 1868.

PART I.—BRORSEN'S COMET.

TEN years ago (this was written in 1869), all that astronomers could hope to do with comets was to note their appearance and changes of appearance when viewed with high telescopic powers. There was one instrument, indeed, the polariscope, which afforded doubtful evidence respecting the quality of the light we receive from comets, and thus allowed astronomers to form vague guesses respecting the structure of these mysterious wanderers. But beyond the unsatisfactory indications of this instrument, astronomers had no means whatever of ascertaining the physical nature of comets.

At present, however, an instrument of incomparably higher powers is applicable to the inquiry. The spectroscope has the power of revealing, not only the general character of any substance which is a source of light, but even of exhibiting, in many instances, the elementary constitution of such a substance. The indications of this wonderful instrument of analysis are not affected by the distance or dimensions of the object under examination. So long as the object is luminous the spectroscope will tell us with the utmost certainty whether the light is inherent or reflected; and if the light is inherent—that is, if the object is self-luminous—the spectroscope will tell us with the utmost certainty what terrestrial elements (if any) exist in the constitution of the object. It is with the revelations of the spectroscope respecting Brorsen's comet that I now propose to deal. I must make a few preliminary remarks, however, respecting the various peculiarities of structure which have been presented by comets.

I assume that my readers are familiar with the general appearance presented by comets—at least by those which are visible to the naked eye. It may be necessary to note, however, of the three features commonly recognised in comets—viz. the *nucleus*, *coma*, and *tail*—the coma alone is invariably exhibited. A comet which has neither nucleus nor tail presents simply a round mass of vapour slightly condensed towards the centre. The nucleus, when seen, appears as a bright point within the condensed part of a comet. The tail, as every one knows, is a long train of light issuing from the head.

It was noted in very early times that comets are almost perfectly translucent. This peculiarity has been confirmed by modern and more exact observations. Sir W. Herschel watched the central passage of a comet over the fainter component of a double star; and he could detect no diminution of the star's brilliancy. Similar observations were made by MM. Olbers and Struve. Sir John Herschel watched the passage of Biela's comet over a small cluster of very faint telescopic stars. The slightest haze would have obliterated the cluster, yet no appreciable effect was produced by the interposition of cometic matter having a thickness (according to Herschel's estimate) of 50,000 miles. And there is another remarkable evidence of tenuity. From recognised optical principles, a star seen through the globular head of a comet, should appear displaced from its true position just as any object seen (non-centrally) through a globular decanter full of water seems thrown out of its true place. The astronomer Bessel made an observation on a star which approached within about eight seconds of the nucleus of Halley's comet, and he found that the place of the star was not affected to an appreciable extent.

Whether the nucleus of a comet is solid or not had long been a disputed point among astronomers. With telescopes of moderate power the bright point within the coma presents an appearance of solidity which might readily deceive the observer. But with an increase of power the nucleus assumes a different appearance. Instead of having a well-defined outline, it appears to merge into the coma by a somewhat rapid gradation—but not by an abrupt variation—of light. Good observers have reported the extinction of telescopic stars behind the nuclei of comets, but there are peculiar difficulties about an observation of this sort; and it is very difficult to determine whether a star is really concealed from view by the interposition of the nucleus or simply obliterated by the glare of light.

The tail of a comet appears nearly always as an extension from the coma, and a dark interval is usually seen between the head and the tail. But there is an immense variety in the configuration of comets' tails.

The comet of 1744 had six tails spread out like a fan. The comet of 1807 had two tails—both turned from the sun. The comet of 1823 had also two tails, but one was turned almost directly towards the sun. Other comets have had lateral tails.

The processes which seem to be passed through by comets during their approach towards and recession from the sun have proved very perplexing to astronomers and physicists. When first seen a comet usually appears as a light roundish cloud with a point of brighter light near the centre. As it approaches the sun the comet appears to grow considerably brighter on the side turned towards him. An emanation of light seems to proceed towards the sun for a short distance and then to curl backwards and stream out in a contrary direction. Gradually the backward streaming rays extend to a greater distance—the nucleus continuing to throw out matter towards the sun. Thus the tail is formed; and it is often thrown out to a distance of many millions of miles in a few hours.

One of the most singular facts connected with the approach and recession of a comet, is the peculiarity that the comet grows gradually smaller and smaller as it approaches perihelion, and swells out in a corresponding manner as it passes away from the sun. The comet of 1652 was observed by Hevelius to increase so rapidly in dimensions as it passed away from the sun, that between December 20 and January 12 its volume had increased in the proportion of about 13,800 to 1.

When it was last visible this comet exceeded the sun in volume. This observation, on which much doubt had been thrown, has been confirmed by the researches of the best modern observers. M. Struve measured Encke's comet as it approached the sun towards the end of the year 1828. He found that between October 28 and December 24 the comet collapsed to about the sixteen-thousandth part of its original volume. Sir John Herschel found in like manner that Halley's comet when passing away from the sun increased in volume upwards of fortyfold in a single week.

The tremendous heat to which many comets are subjected during perihelion passage is an important point for consideration, in attempting to form an opinion of the physical structure of comets. Newton calculated that the comet of 1680 was subjected to a heat 2,000 times greater than that of red-hot iron. But comets have been known to approach the sun even more closely. Sir John Herschel estimates that the comet of 1843 was subjected to a heat exceeding in the proportion of 24½ to 1 the heat concentrated in the focus of Perkins' great lens. Yet the heat thus concentrated had sufficed to melt agate, rock-crystal and cornelian.

We cannot wonder that so great an intensity of heat should have produced remarkable effects upon many comets. The great wonder is that any comet should resist the effects of such heat without being dissipated into space.

We learn from Seneca that Ephorus, an ancient Greek author, mentions a comet which divided into two distinct comets. Kepler considered that two comets which were seen together in 1618 had been produced by the division of a single comet. Cysatus noticed that the great comet of 1618 showed obvious signs of a tendency to break up into fragments. This comet when first seen appeared as a circular nebulous cloud. A few weeks later it seemed to be divided into several distinct cloudlike masses. On December 20 'it resembled a multitude of small stars.'

We might doubt whether these observations were entitled to credit, were it not that, quite recently, Biela's comet has been seen to separate into two distinct comets, each having a nucleus, coma, and tail, and each of which pursued its course independently until distance concealed both from view.

It is clear that nothing but a long series of careful observations can put us in a position to theorise with confidence, respecting the nature of comets, the processes of change which they undergo, and the functions which they subserve in the economy of the solar system. We may therefore dwell with particular satisfaction on the fact that every comet which has appeared during the last two years has been subjected to careful observation, and that at length, by means of spectroscopic analysis, we are beginning to get hold of positive facts respecting comets, and have promise of shortly being able to form consistent theories with regard to these singular members of the solar system.

I have had occasion in other works to speak of the principles on which spectroscopic analysis depends;

but I think it best briefly to restate the most important points. When the light from a luminous object is received upon a prism, there is formed what is called the prismatic spectrum. According to the nature of the source of light this spectrum varies in appearance. If the source of light is an incandescent body the spectrum is a continuous, rainbow-tinted streak. Where the light comes from an incandescent mass surrounded with cooler vapours, the streak of rainbow-coloured light is crossed by dark lines whose position indicates the nature of the vapours which the light has traversed. When the light comes from luminous vapours the spectrum consists wholly of bright lines; and these have exactly the same position as the corresponding dark lines which are seen when the same vapours intercept light from an incandescent solid mass. Lastly, when light is reflected from an opaque substance, the spectrum is the same as that which would be presented by the light *before* reflection, unless the opaque substance is surrounded by vapours, in which case the spectrum will be crossed by new dark lines corresponding to the absorptive qualities exerted by those particular vapours.

We see then the wonderful qualities of the new analysis. Applied to the sun and stars it has enabled our physicists and astronomers to pronounce as confidently that certain elements exist in these far distant orbs, as the chemist can pronounce on the constitution of substances submitted to his direct analysis. The questions, or some of them, which have been at issue respecting comets will undoubtedly yield to the powers of the spectroscope. The great want, at present, is a brilliant comet to work upon. Donati's comet (1859), or the great comet of 1861, would have served this purpose admirably, but the first came in the very year in which the principles of spectroscopic analysis were first discovered; and the powers of the spectroscope were only just beginning to be recognised when the comet of 1861 made its brief visit to our northern skies.

Two small comets have been analysed with the spectroscope, and each presented similar results. The spectrum in each case consisted of thin bright lines on a faint continuous streak of light. And from the fact that the bright lines did not extend across the whole breadth of the faint streak of light, it became evident that they formed the spectrum of the nucleus, the faint continuous spectrum belonging to the coma. Hence it resulted that the nucleus of each of these small comets consisted of self-luminous gas, while the coma either consisted of incandescent solid matter or shone by reflecting the light of the sun. The latter is far the more probable hypothesis. In fact, when we consider the extreme tenuity of the substance of a comet, and therefore the certainty that if composed of solid matter such matter must be dispersed in very minute fragments, we shall recognise the extreme improbability that these fragments should be self-luminous through intensity of heat. If the comets had been brighter, I may remark, there would have been no dubiety respecting this point, since it would have been possible to compare the continuous streak of light with the solar spectrum, and, by the resemblance or dissimilarity of the two spectra, to determine whether the coma really shines by reflecting the sun's light or not.

Brorsen's comet has now been examined with the spectroscope, and with results quite different from those which attended the analysis of the other two. Dr. Huggins, the physicist, who examined the latter, says of Brorsen's comet:—

'It appears in the telescope as a nearly round nebulosity, in which the light increases rapidly towards the centre, where, on some occasions, I detected, I believe, a small stellar nucleus. Generally, this minute nucleus was not to be distinguished in the bright central part of the comet. The spectrum consists for the most part of three bright bands. The length of the bands in the instrument shows that they are not due alone to the stellar nucleus, but are produced by the light of the brighter portions of the coma. I took some pains to learn the precise character of these luminous bands. When the slit was wide they resembled the expanded lines seen in some gases. As the slit was made narrow the two fainter bands appeared to fade out without becoming more defined. I was unable to resolve them into lines. The middle band, which is so much brighter than the others that it may be considered to represent probably three-fourths, or nearly so, of the whole of the light which we receive from the comet, appears to possess similar characters. In this band, however, I detected occasionally two bright lines which appear to be shorter than the band, and may be due to the nucleus itself. . . . Besides these bright bands there was a very faint continuous spectrum.'

Interpreting these observations according to the principles which have been already stated, we deduce the following interesting results.

The *nucleus* of Brorsen's comet consists of luminous gas. The *coma* is also gaseous in the neighbourhood of the nucleus, but its outer portions are of a different character and shine by reflecting the solar light. This part of the coma may be either liquid or solid. There is nothing opposed to the supposition that it is of the nature of cloud—that is, that it is produced by the condensation of true vapour into minute liquid globules.

Returning to the consideration of the gaseous part of the comet the question will at once suggest itself what the gases may be which constitute the substance of the nucleus and coma. Here our information is not quite so satisfactory as could be desired.

The brightest band is in the green part of the spectrum, and agrees very nearly with the brightest line in the spectrum of nitrogen. The want of exact agreement prevents us from assuming that nitrogen really exists (in any form) in the substance of the comet. The other lines of the spectrum of nitrogen are not present in the spectrum of the comet: but this peculiarity is not so perplexing as the other, for it is well known that certain lines will disappear from the spectra of hydrogen, nitrogen, and other gases, under particular circumstances of illumination, temperature, and so on.

Nor is the circumstance that there are bands of light instead of well marked lines a peculiarity which need cause perplexity. For under certain circumstances of temperature and pressure, the lines of the spectra of various gases become expanded or diffused until they appear as bands of light.

The two fainter bands are yellow and blue respectively. They cannot be identified with the lines seen in the spectra of any known terrestrial gases.

Of whatever gases the nucleus is composed it appears that conditions wholly different from any with which we are familiar on earth prevail in this, and doubtless in all other comets. The gases which form the nucleus, though self-luminous, are probably not incandescent. Remembering that comets are luminous when situated far out in space beyond the orbit of our own earth, we are prevented from assuming the existence of an intensity of heat (due to solar action) sufficient to account for their inherent light. And if the light of a comet were due to a state of incandescence in the component gases, there would be a rapid consumption of the substance of the comet, and we should be quite unable to account for the fact that Halley's comet has continued to shine, with no appreciable loss of brilliancy, for upwards of three hundred years. We seem forced, therefore, to surmise that the gases which form the substance of comets owe their light to a species of phosphorescence which is independent of the comet's temperature, or else to some electrical properties the nature of which it would not be easy to divine.

Our perplexity is increased when we see the gases which form the nuclei assuming either the liquid or the solid form in the outer part of the coma. The change from gaseity to liquidity or solidity is an evidence of loss of heat, whereas one would expect the outer part of the coma, which is exposed to the full intensity of the sun's action, to be the most heated portion of a comet's volume.

None of the comets which have been examined have had a tail, so that we are unable as yet to form any certain opinion respecting the nature of this portion of a comet's volume. It seems *almost* certain, however, that the tail shines by reflected light, because in every known instance the tail has appeared as an extension from the outer part of the coma, and may therefore be expected to resemble that portion of the comet in its general characteristics.

One of the comets which has been examined with the spectroscope, though it has not a visible tail, has been shown to have an appendage of a very remarkable character, respecting which, also, we have been able to learn several interesting particulars.

In the year 1866 a telescopic comet was discovered by M. Tempel. This was the first comet examined by Dr. Huggins. Its orbit was carefully calculated by the German astronomer Oppolzer, and found to pass very near the orbit of our own earth. Soon after this, Professor Adams calculated the orbit of the November shooting-stars; and to the surprise of the astronomical world it was found that these minute bodies travel along the very path in space which had been already assigned to Tempel's comet. We need not here discuss the circumstances of this discovery. Let it suffice to state that all astronomers who are competent to form an opinion on the subject are agreed that the November shooting-stars are certainly due to the existence of a long-extended flight of cosmical bodies travelling in the track of Tempel's comet.

Now it appears clear that this flight of cosmical bodies may be looked upon as constituting an extension of the comet—an invisible train, as it were. But for the accident that the comet's track intersects the earth's path in space, we should have remained for ever ignorant of the fact that the comet has any other extent than that which is indicated by its telescopic figure. Now, however, that we know otherwise, we recognise the probability that other comets which have been looked upon as tailless may have invisible extensions reaching far behind them into space, or even completely around their orbit.

But the members of the November shooting-star system have been subjected to spectroscopic analysis. We know that they contain several terrestrial elements; and we recognise the probability that if we could examine one of them before its destruction (in traversing our own atmosphere) we should find a close resemblance in its constitution to that of those aërolites or meteorites which have reached the surface of the earth.

But here we encounter a new difficulty. One theory respecting the tails of comets has accounted for them by the supposition of a propulsive effect exerted by the solar rays; and another theory has ascribed them to the action of vapours ascending in the solar atmosphere. But if the tails of comets really consist of solid matter very widely dispersed, it must be quite evident that neither of these causes could suffice to account for the great extension of these appendages. Then the rapid manner in which the tails seem to be formed remains wholly mysterious. And we are also left without any explanation of the rapid change of position exhibited by the tail while the comet is sweeping around the sun at the time of nearest approach to that luminary. Sir John Herschel compared this motion to that of a stick whirled round by the handle—the whole extent of the tail partaking in the movement as if comet and tail formed a rigid mass.

The difficulties here discussed seem in the present state of our knowledge wholly insoluble. In fact, it seems impossible even to conceive of a solution to the last mentioned phenomenon, so long as we look upon the comet's tail as a distinct *unvarying* entity. For instance, if the tail, a hundred millions of miles long, which extended backwards from Halley's comet *before* perihelion passage, consisted of the same matter as the tail which projected forwards to the same extent a few days later, then certainly there is nothing in our present experience of matter and its relations which can enable us to deal with so astounding a phenomenon. It will be understood, of course, Sir John Herschel does not say in so many words, that the tail of Halley's comet was brandished round in the manner described above, but that, although it appeared to move in this manner, it is impossible so to conceive of its motion.

We refrain, however, from speaking further on a point respecting which we have no means of reasoning satisfactorily. Mere guess-work is an altogether unprofitable resource in the discussion of scientific matters.

Now that we have so powerful an instrument of research as the spectroscope, there really seems hope that even the hitherto inscrutable mysteries presented by comets' tails may one day be interpreted. Each comet which has been subjected to spectroscopic analysis has revealed something new. Observations, such as those which have been made on Brorsen's comet, and on the two telescopic comets previously examined by Dr. Huggins, are not merely valuable in themselves, but as affording promise of what may be achieved when some brilliant comet shall be subjected to spectroscopic analysis. When we consider that all the comets yet examined have been absolutely invisible to the naked eye on the darkest night, whereas several of the great comets have blazed forth as the most conspicuous objects in the heavens, and have even been visible in the full splendour of the midday sun, we see good reason for the hope that far fuller information will be gained respecting the structure of comets so soon as one of the more important members of the family shall have paid us a visit.

Whenever such an event may happen it is not likely to find our spectroscopists unprepared. It is probable that, before long, every important observatory will be supplied with spectroscopes. Already some of the most powerful telescopes in use have been fitted with them. We hear also, that the giant reflector of the Parsonstown Observatory—commonly known as the Rosse telescope—has been armed with a spectroscope especially constructed for the purpose by Mr. Browning, F.R.A.S., the optician. Not only in England, but at the principal Continental observatories, spectroscopic work is in progress, and observers are daily becoming more and more familiar with the powers of the new analysis. Stars which are far too small to be viewed by the naked eye have already been examined with the spectroscope. The Padre Secchi at Rome has just published a list of minute red stars thus examined. It is such delicate work as this which will fit observers to deal with the difficulties involved in the spectroscopic analysis of comets.

We shall see when we come to deal with the second comet of the year 1868, that we have yet better reason than the analysis of Brorsen's comet has afforded for hoping that before long we may have interesting and exact information respecting the structure of these mysterious wanderers.[1] We may even hope to gain some knowledge respecting the purposes which comets subserve in the economy of the solar and sidereal systems.

PART II.—WINNECKE'S COMET.

IN the preceding pages I have described the principal features presented by comets as they approach and pass away from the neighbourhood of the sun. The various hypotheses which have been put forward to account for these peculiarities must now for a brief space claim our attention. Although we are far from being in a position to theorise with any confidence respecting the nature of comets, and still less as to the purposes which they subserve in the economy of nature, yet the observations made upon the second comet of the year 1868 have resulted in a positive discovery which may serve as a standpoint, so to speak, whence we can examine somewhat more confidently than of old, the various theories which have suggested themselves to those who have studied cometic phenomena.

In considering these hypotheses we have to distinguish between the views which have been entertained respecting the *nucleus* and *coma*, and those which regard the less intelligible phenomena presented by the *tail.* This remark may seem trite and obvious, but in reality the two classes of hypotheses are found singularly confounded together in many works on popular astronomy. Let it be understood, then, that when, in speaking of an hypothesis respecting comets, no special mention is made of the tail, it is to be assumed that the hypothesis applies solely to the head of the comet. The same holds, by the way, with reference to the phenomena presented by comets. For instance, when we said in the paper on Comet I. that comets grow smaller as they approach the sun, the remark was to be understood to apply to the volume of the head, not to the whole space occupied by the head and tail. In fact, it would have been impossible to assert anything with respect to the volumes of comets' tails, inasmuch as the apparent extent of these appendages varies according to the atmospheric conditions (humidity, clearness, and so on) under which the comet is observed, and also according to the light-gathering power of the observer's telescope.

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[1] I have left these passages unaltered. It must be remembered, however, that they were written in 1869. Since then, besides several small comets, the comets of 1874 and of the present year (1881) have been examined with the spectroscope. Thus far, all comets so examined have shown, besides faint continuous spectra indicating that they shine partly by reflected light, either the gaseous spectrum of Brorsen's comet, or that of Winnecke's, as described in the text.

To return, however, to the theories which have been formed respecting comets.

It has been commonly admitted that the substance of which comets are composed is either wholly or principally gaseous. In no other way, it should seem, can the remarkable variations of appearance which comets present as they approach the sun or recede from him be reasonably accounted for.

Kepler held that comets are wholly gaseous, and that they are liable to be dissipated in space by the sun's action. He supposed that the process of evaporation which thus led to the destruction of a comet was carried on through the medium of the tail. It need hardly be said that modern observations are completely opposed to this view. Comets have been seen to return again and again to the neighbourhood of the sun without any apparent diminution of volume, although at each return a tail of considerable length has been thrown out. For a long time, indeed, it was thought that Halley's comet was gradually diminishing in volume; but at the last return this magnificent object had recovered all its pristine splendour.

Newton held, on the contrary, that comets are partly composed of solid matter. He supposed that only the gaseous matter was affected to any noteworthy extent by the action of the sun's heat. Raised from the solid nucleus the vaporised particles passed first into the coma, he imagined, and were thence carried off into space to form the comet's tail. Others so far modified Newton's views as to suggest that the vaporised matter is not wholly carried off, but partially re-precipitated upon the head of the comet, just as the vapours raised from the ocean are precipitated upon the earth in the form of rain.

We have seen that a comet diminishes in volume as it approaches the sun. It will be noticed that both the theories which have been described would account satisfactorily for the observed decrease of volume. But neither of them gives any satisfactory explanation of the fact that a comet recovers its original volume as it departs from the sun's neighbourhood. Newton, indeed, put forward certain views respecting the emission of smoke from the nucleus during perihelion passage, and he surmised that the true dimensions of the comet might in this manner be veiled to a certain extent; but this part of his theory has the disadvantage of being almost unintelligible, besides being wholly insufficient to account for the *regular* diminution and increase which attend the approach and recession of a comet.

A theory has lately been put forward by M. Valz which accounts for the variation of a comet's volume by the supposition that the solar atmosphere exerts a power of compression, which, varying with that atmosphere's density, is most effective in the sun's neighbourhood. We know, for instance, that a balloon must not be fully inflated at first rising, because when it reaches the upper regions of air, where there is less compression, the enclosed gas expands, and would burst the silk if the balloon had been fully filled at first. And certainly, on the somewhat bold assumption that the solar atmosphere extends outwards to those regions in which the observed change of volume takes place, and on the additional and equally bold supposition that comets are surrounded with a film of some sort performing the same function as the silk of the balloon (or that in some other way the substance of the comet is prevented from intermingling with the substance of the solar atmosphere), the theory of M. Valz would have a certain air of probability. Even then, however, it would be insufficient to account for the enormous extent to which the variation has been observed to proceed.

The only probable explanation of the variation in question is that which is put forward by Sir John Herschel in his admirable work on the southern heavens. During his stay at the Cape of Good Hope he had an opportunity of observing the recession of Halley's comet, and he discusses the phenomena with admirable acumen and judgment. The result at which he arrives appears to afford a simple and rational explanation of the observed phenomena. He supposes that as a comet approaches the sun the action of the solar heat transforms the nebulous substance of the comet into invisible vapour. This action progressing from without inwards, of course produces an apparent diminution of volume. The diminution continues as long as the comet is approaching the sun, and for yet a few days after perihelion passage; but soon after the comet has begun to leave the sun's neighbourhood the transparent vapour begins to return to its original condition, the solar action being insufficient to keep the whole of the vaporised matter in the gaseous state. Thus the comet gradually resumes its original apparent dimensions.

There are few phenomena which have given rise to more speculation than those presented by the tails of comets. Astronomers, who, in dealing with other matters, have exhibited the soundest judgment, and the most logical accuracy of argument, seem to feel free to indulge in the most fanciful speculations when dealing with this subject.

A favourite theory with the earlier astronomers was founded on the observed peculiarity that the tails of comets are usually turned directly from the sun. It was supposed that the tail is not a really existent entity, but merely indicates the passage of the solar rays through space, after their condensation by the spherical head of the comet. Just as a light received into a dark room through a small aperture appears as a long ray extending in a straight line through the room, so, according to this theory, the sun's light, concentrated by the comet's head, throws a long luminous beam into space. Unfortunately for this view there is a want of analogy between the two cases thus brought into comparison. The light shining into a room produces the appearance of a ray because it illuminates the air and the small particles of floating dust which it encounters in its passage. There is nothing corresponding to this in the interplanetary spaces. If there were, the sky would never appear black, since the sun would always be shining on matter capable of reflecting his rays.

Kepler was the first to form a reasonable hypothesis respecting comets' tails. He supposed that the action of the solar heat dissipates and breaks up a comet's substance. The rarer portions are continually swept away, he imagined, by the propulsive energy of the solar rays, and are swept in this way to enormous distances from the comet's head. The denser portions remain around the nucleus and form the coma.

The modern theory respecting light (according to which there is no propulsion of matter from the sun, but a simple propagation of wave-like motion) does not affect Kepler's hypothesis so much as might be imagined. Whatever theory of light we adopt we are forced to assume an extreme tenuity in the matter which forms the tails of comets. And when once we have made this assumption, we are enabled to admit that even the propagation of a wave-like motion through the ether which is supposed to occupy the interplanetary spaces, might suffice to carry off the attenuated nebulous matter with tremendous rapidity.

The defect of Kepler's theory is that it appears insufficient to account for those anomalous tail-formations which were referred to in our paper on Comet I.

Newton's hypothesis respecting comets' tails was somewhat different. He supposed that the intensely heated comet communicated its heat to the surrounding ether, which thus grew rarer and ascended in the solar atmosphere—that is, flowed away from the sun—precisely as heated air ascends from the earth. The ether thus displaced would carry away with it the rarer portions of the comet's substance, just as smoke is carried upwards by a current of heated air.

It will be seen at once that Newton's theory, like Kepler's, affords no explanation of lateral tails, or of tails turned towards the sun.

In modern times a theory has been founded on the supposition that cometic phenomena may be due to electrical agency. The German astronomer Olbers was one of the first to propound this view, and many eminent astronomers—amongst others the younger Herschel—have looked with favour upon the theory. As yet, however, we do not know enough respecting electricity to accept with confidence any theory of comets founded upon its agency.

The comet respecting which I now have to treat was discovered in the middle of June 1868, by Winnecke. At first it was a telescopic object, but it gradually increased in brilliancy until it became visible to the unaided eye. In the telescope, at the end of June, the comet appeared as a circular cloud rather brighter in the middle, where there was a roundish spot of light. A tail could be traced to a distance of about one degree from the nucleus.

Dr. Huggins quickly subjected the new arrival to spectroscopic analysis. The result, at first sight, seemed to differ little from that which had been noticed in the case of Brorsen's comet. Indeed, the astronomers at the Paris Observatory and the Padre Secchi at Rome were led to pronounce the spectra of the two comets to be absolutely identical. The more powerful spectroscopic appliances employed by Dr. Huggins, however, exhibited important differences.

The spectrum consisted of three bands of light separated by dark intervals. Of these bands two were greenish blue, the other greenish yellow. The two former were tongue-shaped, the last was narrowed off at both extremities.

From what I have said above respecting the nature of spectroscopic analysis, it will be understood that the distribution of the comet's light along the length of the spectrum is the most important circumstance to be attended to in endeavouring to form an estimate of the substance of the comet. But as we see that there are, in this instance, peculiarities affecting the breadth of the spectrum, it will be well briefly to consider their meaning. The matter is, in reality, simple enough, but requires a little attention.

The breadth of the spectrum corresponds to the breadth of the object which is the source of light. If that object is uniformly bright the spectrum is also uniformly bright across its breadth, whatever variations may exist in the direction of its length. But if the object is brighter in some parts of its breadth than in others, the spectrum will show corresponding variations of brilliancy across its breadth. Hitherto we have been assuming that all the light from the object is of the same kind, however it may vary in brilliancy. Suppose, however, that the light from the middle of the object gives one kind of spectrum, the light from the outer parts another; then the spectrum will vary in *character* as well as in brilliancy across its breadth. Suppose, for example, that the middle of the object is gaseous while the outer parts are solid or liquid, then the appearance presented would be two thin streaks of rainbow-tinted light, separated by a dark space[1] across which would be seen the bright lines belonging to the gaseous central part of the luminous object.

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[1] Our readers will, of course, understand that a *slice* only of the object is brought under spectroscopic analysis at once. If the *whole* of a circular object, whose centre was gaseous, were examined at once, the middle streak of the spectrum would exhibit the compound spec-

Now the breadth of the spectrum seen by Dr. Huggins corresponded with the breadth of the coma so far as the widest parts of the tongue-shaped bands were concerned. But the narrower parts were about the width of the nucleus. Therefore the first question to be decided was this,—Is the narrowing of these bands of light towards one extremity, and of the other towards both extremities, to be considered as indicative of any difference in *character* between the light emitted by the nucleus and that emitted by the coma? At first sight it seems that no other conclusion could be come to. But a little consideration enabled Dr. Huggins to arrive at a different result. The tongue-shaped bands were not only narrower but very much fainter towards one end. They were also fainter along their outer edges, on account, of course, of the faintness of the coma as compared with the nucleus. Now it was possible that the narrowing down of the bands might be only apparent, and due to the fact that their outer parts, though really existent, became invisible at the fainter end. And there were two modes of attacking the question. First, the observer could determine by a careful inspection whether the light at the narrower end of the tongues was so faint that it *ought* to disappear at the edges merely by undergoing the same sort

trum of the edge and centre of the object. Such an arrangement would clearly be unfavourable to the formation of clear views respecting the character of the object's light.

of reduction as the brighter light at the broader end of the tongue: this would show that the coma does not differ in constitution from the nucleus. Secondly, if the strip brought under examination were narrowed by any contrivance, it is clear that any difference which might exist in the constitution of the coma and of the nucleus ought to be exhibited in a more marked manner.

Dr. Huggins applied both methods, and each resulted in showing that the nucleus has the same constitution as the coma, excepting only that the exterior part of the coma seems to give a continuous spectrum. In other words, the nucleus and all the coma except its outer shell consist of the same incandescent vapour; but the outer shell of the coma either consists of incandescent solid or liquid matter, or shines by reflecting the solar rays.

So far, however, there is little in the spectroscopic analysis which differs in character from what had been observed respecting Brorsen's comet. But we have now to record one of the most startling discoveries ever made respecting comets.

Dr. Huggins was reminded by the appearance of the cometic spectrum of a form of the spectrum of carbon which he had observed in the year 1864. It must be premised that the spectrum of an element often assumes a different form according to the circumstances under which it is obtained. Amongst the objects which have spectra thus variable is the element carbon. The particular form of carbon-spectrum which resembled that of the comet is that obtained when an electric spark is taken through olefiant gas—a substance which, as many of my readers are doubtless aware, consists of carbon and hydrogen, and is one of the constituents of ordinary coal gas.[1] Of course the spectrum of olefiant gas exhibits the bright lines belonging to hydrogen; but as these are well known, the part of the spectrum belonging to carbon also becomes determinable.

Having noticed, as we said, the resemblance between the spectrum of the comet and a form of the carbon spectrum, Dr. Huggins determined to compare the two spectra directly. We have not space to explain the contrivances by which this was effected. Suffice it to say, that when the two spectra were brought side by side it appeared that in place of mere resemblance there was absolute identity. The bands of light which formed the comet's spectrum were found not only to coincide in position with those which appeared in the spectrum of olefiant gas, but to present the same relative brightness. Two days later the observations were repeated by Dr. Huggins in company with Professor Miller (who had been associated with him in his earlier spectroscopic labours), and both observers agreed in the opinion that the coincidence between the spectra could not be more exact.

The reader will, of course, understand that the hydrogen lines belonging to the spectrum of olefiant gas are not seen in the spectrum of the comet.

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[1] The other constituent is 'fire-damp,' also a compound of carbon and hydrogen. Olefiant gas is commonly called heavy carburetted hydrogen, while fire-damp is termed light carburetted hydrogen.

Now only one interpretation can be put on this remarkable result, and that is that Winnecke's comet consists of the incandescent vapour of carbon,—*not* of burning carbon, be it understood, but of *volatilised* carbon.

But carbon, as we are acquainted with it on earth, is a substance whose chief peculiarity, perhaps, is its fixity at ordinary temperatures; and no phenomenon hitherto presented by comets is more perplexing than the existence of volatilised carbon as the main or only constituent of a comet of enormous real bulk, when that comet was not so near to the sun as to be raised (one could suppose) to an extraordinarily high temperature. There have been cases where comets have been so near to the sun as to account for almost any conceivable change in the constitution of their elements. An intensity of heat of which we can form no conception must have been experienced, for example, by Newton's comet; and a still fiercer heat dissipated the substance of the comet of 1843. But Winnecke's comet at the time of observation was at far too great a distance from the sun for us to assign to its mass a temperature which under ordinary circumstances would account for the volatilisation of carbon.

Nor does the rarity of the atmosphere in which the comet was moving serve to help us in our difficulty. Doubtless we are little familiar with the effects which terrestrial elements would experience if they were distributed freely in the ether occupying the interplanetary spaces. But so far as our experience enables us to judge, we should rather look for intensity of cold than of heat under such circumstances. We see the heights of the Andes and of the Himalayas clothed in perpetual snow, though day after day the fierce heat of the tropical sun pours down upon them, and though there is no winter there (in our sense of the word) during which the snows are accumulated. We know that the explanation of this peculiarity lies in the extreme rarity of the air at a great height. It seems, therefore, reasonable to conclude that the cold of the interplanetary spaces must be far greater. Yet here we have an object whose light comes from the incandescent vapour of so fixed and unchangeable a substance as carbon, and thus, in place of an almost inconceivable intensity of cold, we find the evidence of intense heat.

It seems impossible, at present, to suggest any explanation of the observed phenomena. That carbon exists out yonder in space in the state of luminous gas or vapour, is the one fact of which alone we can be certain. Dr. Huggins in his treatment of this fact suggests the possibility that the carbon may be divided into particles so minute, that as the comet approaches the sun, more of the sun's heat is gathered up, so to speak, and that thus the carbon is volatilised. He also points to phenomena of phosphorescence and fluorescence in illustration of the appearance presented by the comet's spectrum, but without suggesting any association between these phenomena and those presented by comets.

One cannot help associating the new views thus opened out to us respecting comets, with the discovery recently made that the meteoric bodies which flash singly or in showers across our skies belong in reality to the trains of comets. We have now every reason to believe that there is not a single member of the meteoric systems, not a single aërolite, bolide, or fire-ball, that has not belonged once upon a time to a comet. The evidence on which this view is founded, though it may seem insufficient at a first glance, is almost irresistible to those who can appreciate its significance. Let us briefly recapitulate the facts.

It has been proved that shooting-stars come from the interplanetary spaces, that they form systems, and that these systems travel in regular elliptical orbits about the sun. Two of the systems produce striking meteoric displays, viz. the system encountered by the earth on or about August 10, and the system encountered on or about November 13. Now it had been suggested that the members of the former system follow the track of the conspicuous comet which made its appearance in the year 1862; and it was proved that, assuming the orbit of the meteors to be very eccentric, and assigning to them a period of 147 years (that of the comet), their motions corresponded in the most remarkable manner with the orbital track of the comet. In fact, the agreement was so close that very few who had examined the question could believe it to be accidental. But there were two objections on which some stress was laid. First, it had been necessary to make assumptions respecting the motion of the meteors;

secondly, those assumptions were not rendered probable by anything which had been *proved* respecting any meteoric system. The examination of the November star-shower by a host of eminent mathematicians in 1866–67 led to results which at once removed these objections, and brought new evidence—and that of the most striking character—in favour of the theory that comets and meteors are associated. It had been supposed that the November meteors travelled in a nearly circular orbit within a period of somewhat less than a year. Adams proved that they travel in an orbit extending far out into space beyond the orbit of distant Uranus. And the period of this orbit was calculated to be 33¼ years. Here, then, was strong confirmatory evidence in favour of the elliptic orbit and the long period assigned, by way of assumption, to the August meteors. But this was far from being all. Astronomers looked for a comet to be associated with the November meteors: and they found one—a small one, it is true, but with a well-defined character—an orbit calculated beyond suspicion of important error, and agreeing so closely in its motions with those of the November meteors that the chances were millions on millions to one against the coincidence being accidental. It hardly required, after this, that an association should be pointed out between other meteor-systems and other comets. Yet this has been done, and thus that which had already been demonstrated was illustrated by new proofs. We may say that nothing which men of science have dealt with has ever been more satisfactorily proved than the fact that meteors are the attendants on comets.

Now, how meteors are thrown off from cometic nuclei we are not yet able to say. They differ wholly in character from their source, and thus we learn that the gaseous nature of cometic nuclei is due to the action of causes connected with those to which the nuclear structure of the comet's head is due. But whether the first formation of meteoric systems is associated in any way with the processes which result in the formation of a comet's tail, is not quite so clear. As yet no comet which has had a brilliant tail has been subjected to spectroscopic analysis, so that we cannot pronounce with any certainty respecting the structure of these singular appendages. Some astronomers are disposed to look on the formation of a track of meteors all round the orbit of a comet as due to the action of influences by which parts of the comet's mass are thrown into orbits of slightly longer period than that of the head, though closely resembling that orbit in figure. Be this as it may, it is certain that the great contrast in character between the meteoric bodies which form the train of a comet, and the gaseous nucleus and coma, remains yet among the mysteries which astronomers have been unable to clear up.

But so soon as it had been shown that a comet's head is formed of a certain well-known terrestrial substance, it was natural that the question should be asked whether this substance is to be found in meteors. Hitherto no great progress has been made in determining the elementary constitution of meteors which have not actually fallen upon the earth. It is so difficult to catch them during their brief transit across our skies that only a few substances, as sodium, phosphorus, magnesium, and so on, have been shown with any appearance of probability to exist in shooting-stars. Certainly carbon is not among the number of those elements which have been detected in this way. But at a recent meeting of the Astronomical Society, it was stated that several aërolites contain carbon in their structure, and Dr. De la Rue offered a fragment of one of these to Dr. Huggins for analysis. Certainly a strange circumstance that an astronomer who had analysed the structure of a body millions of miles away from the earth, should take into his hands and subject to chemical analysis a fragment which had once in all probability belonged to a similar comet!

In conclusion, I must notice that there has been a remarkable absence during the past few years of those brilliant and long-tailed comets which alone seem calculated to afford the spectroscopist the means of answering some of the difficult questions suggested above. The tail of Winnecke's comet was too faint to give a visible spectrum. In fact, the comet itself was only just visible to the naked eye. When a blazing object like Donati's comet or the comet of 1861 comes to be subjected to spectroscopic analysis, we may hope for an amount of information compared with which that hitherto obtained is probably altogether insigficant.

From *Fraser's Magazine* for February and June 1869.

COMETS OF SHORT PERIOD.

IT is related by Apollonius the Myndian, that the Chaldean astronomers held comets to be bodies which travel in extended orbits around the solar system. 'The Chaldeans spoke of comets,' he says, 'as of travellers, penetrating far into the upper celestial spaces.' He adds, that those ancient astronomers were even able to predict the return of comets. How far it may be safe to accept the statements of Apollonius is uncertain. He ascribed other powers to the Chaldeans, of which we may fairly doubt their possession—for instance, the power of predicting earthquakes and floods. In fact, there is so marked a disposition among ancient writers to exaggerate the acquisitions of Chaldean astronomers, that it becomes extremely difficult to distinguish truth from falsehood. Still, there is sufficient evidence of their skill and patience as observers, to render it fully possible that they may have discovered the periodicity of one or two comets.

But until the rise of modern astronomy, the opinion which was almost universally held respecting comets was that of Aristotle, that they are of the same nature as meteors or shooting-stars, existing either in the air not far above the clouds, or certainly far below the moon.

The discovery of the periodicity of Halley's comet following quickly on Newton's announcement of the law of gravitation, led astronomers to examine the orbits of all the comets which became visible, with the hope of finding that some of these bodies may be travelling in re-entering paths. But inasmuch as none of the brilliant comets of whose appearance records had been preserved seemed to have ever revisited the earth save Halley's alone, while even Halley's travelled in an orbit of enormous extent, an orbit which reached out in space more than three times as far as the orbit of the most distant known planet, astronomers held that the only kind of path which they might expect a comet to pursue was a long oval. They accordingly confined their calculations, and limited the invention of new mathematical processes, to the case of very eccentric orbits.

But in 1770 a comet appeared which led astronomers to form wholly new views. No orbit which could be devised (subject to the above-mentioned condition) could be reconciled with the motions of the new arrival. At length the astronomer Lexell discovered that the path of the comet was not an oval of extreme eccentricity, but an ellipse of such a figure that the comet's period of revolution was less than six years. But here a difficulty arose. The comet was sufficiently conspicuous; and it was asked, how could such an object have gone on circulating so rapidly around the sun, and yet have remained undiscovered? A very singular result rewarded the inquiry into this question. It was found that the aphelion of the comet's path lay just outside the orbit of Jupiter; and, further, that when the comet was last in aphelion, Jupiter was quite close to it. Thus it became clear that the comet had been travelling in another, and doubtless much wider orbit, when its motions had brought it into the neighbourhood of the planet Jupiter—the giant of the solar system. The comet had actually approached the planet nearer than his fourth satellite. 'It had intruded,' says Sir J. Herschel, 'an uninvited member into his family circle.'

The result of this close appulse may be readily conceived. Just as Halley's comet, when close to the sun, sweeps rapidly round him—that is, in a sharply curved path—so the new comet's path was sharply bent around the temporary focus formed by the great planet. But just as Halley's comet, after perihelion passage, moves away from the sun, so Lexell's comet, after what may be termed perijovian passage, moved away from Jupiter, and passed again within the sun's attraction. From this time the comet began to follow a new orbit around the sun. This new orbit was an oval of moderate eccentricity, round which the comet travelled in about five and a half years.

At the next return of the comet to perihelion, it was not likely that astronomers would obtain a view of it; for, on account of the odd half-year in its period, it came to perihelion when the earth held a point in her orbit exactly opposite to that which she had occupied at the comet's former perihelion passage; therefore, the comet which before was favourably, was now unfavourably situated for observation.

As the period for the comet's second return approached, astronomers looked out eagerly for its advent. Again and again the heavens were 'swept' for the faint speck of nebulous light which should have announced the return of the wanderer. But days, and weeks, and months passed, until it became certain that either the comet had been shorn of nearly all its former brilliancy, and had thus escaped unnoticed, or that something had happened to deflect it from its course.

The last alternative appeared so much the more probable one, that mathematicians began to examine the path of the comet, to see whether it had approached so near to any disturbing body as to have been driven from its recently adopted orbit. The examination was soon rewarded with success. If we consider the nature of orbital motion, we shall at once see that, so long as Lexell's comet was subjected to no new disturbing attractions, it was compelled, once in every revolution, to return to the scene of its former encounter with the planet Jupiter. This return was fraught with danger to the stability of the comet's motions. So long as Jupiter was not near that particular part of his orbit at which the encounter had taken place, the comet was free to pass the point of danger, and return towards the sun; but if ever it should happen that Jupiter was close at hand when the comet approached his orbit, then the comet would be as liable to have its motions disarranged as at the original encounter. It happened that the period of the comet's motion in its new orbit was almost exactly one-half of Jupiter's period. This was unfortunate; since it clearly follows that, when the comet had revolved *twice*, Jupiter had revolved *once* round the sun. Thus the comet again encountered the planet, with what exact result has never become known; but certainly with this general result, that the comet's movements were completely disarranged. It has never returned to the neighbourhood of the earth.

We may look upon Lexell's as the first discovered comet of short period; for although it was never seen after its first visit, yet nothing can be more certain than that it did actually return once, and that it went twice round its new orbit. Indeed, if it has not been absorbed by Jupiter—a very unlikely contingency—it must still be revolving in space with an orbit which brings it, once in each revolution, to the scene of its former encounters. The *figure* of its orbit may be altered again and again by encounters with Jupiter; but each new orbit *must* traverse this dangerous point. This follows directly from the laws of orbital motion around an attracting centre. A body will continue to revolve in any orbit along which it has once begun to move, unless it is acted upon by some extraneous force. Accordingly, if at any point of its path an extraneous force suddenly disturb its motion, the disturbed orbit cannot fail to pass through the point of disturbance. Thus the body may again fall under the influence of the disturbing agent, and be caused to move in yet another orbit through the same point. And in the course of millions of years, a body might thus travel in a hundred different orbits, all passing through a common point. There is, indeed, *one* way in which Lexell's comet might have escaped from Jupiter's control. If, after one of its encounters with Jupiter, it happened to pursue a path which brought it very nearly into contact with Saturn or some other large planet, it might be compelled thenceforth to abandon its allegiance to Jupiter. But the probability of this happening to a comet which had once got into the toils may be reckoned 'almost at naked nothing.'

We have been careful to dwell on this point for a reason which will appear presently.

The search for Lexell's comet led to the discovery of a considerable number of nebulæ; and the discovery of nebulæ led in turn to the discovery of another comet of small period. In 1786 Méchain announced to Messier (who had constructed a list of 103 nebulæ) that he had discovered a nebulous object. This turned out to be a telescopic comet. It was again seen by Miss Caroline Herschel in 1795, by Thulis in 1805, and by Pons in 1818. All this time no suspicion had arisen that these observers had seen the same object. But in 1818 the comet remained in view so long that it became possible to calculate its orbit. This was done by the German mathematician Encke, who found that the orbit is an ellipse, and the period of revolution about three years and four months. He found, after a laborious process of calculation, that it could be no other than the object that attracted attention in 1786, 1795, and 1805. Encke then applied himself to calculate the next return of the comet, which he did so successfully that astronomers have continued to call by his name the object whose motions he had been the first to interpret.

Encke's comet was seen by one observer only in 1822, as it was not favourably situated for observation in the northern hemisphere—that observer was M. Rümker, who followed the comet for three weeks at the private observatory of Sir T. M. Brisbane at Paramatta. In 1825, the comet was detected by several independent observers. It was seen again in 1828, being detected by two observers—Harding at Göttingen, and Gambart at Marseilles. In 1832 and 1835, it was seen from the observatory at the Cape of Good Hope.

At the next return of the comet, which took place on December 9, it was visible to the naked eye for the first time since its discovery. At this passage also, a very noteworthy peculiarity was remarked—or rather a peculiarity which had been remarked by Encke in 1818, was now, for the first time, placed beyond a doubt. Encke had suspected that the comet's period was slowly diminishing. Each return to perihelion occurred about two and a half hours before the calculated time. Such a discrepancy may appear very trifling, and in fact it might seem that no certainty could be felt respecting it; and this is the case so far as one or two revolutions are concerned. But when each successive revolution shows the same discrepancy, the deficiency soon mounts up to a period respecting which no doubt can be entertained. For example, between the perihelion passage in 1789 and that of 1865, the comet has made twenty-three revolutions, and each has been less than the preceding by two and a half hours (on the average). Hence, the last revolution of the series occupied two days and a half less than the first. But even this does not express the full effect of the change; for the comet having gained two and a half hours in the first revolution, five in the next, seven and a half in the next, and so on—it is the *sum* of all these gains (and not the gain made in the last revolution) which expresses the total gain of the comet in point of time. Hence the last perihelion passage occurred twenty-nine days before the time at which it would have taken place, but for some unknown cause which has interfered with the comet's motion. What that cause may be, has not yet been certainly determined; but it is at least highly probable that Encke has assigned the true cause in suggesting that so light a substance as the comet may be retarded in its passage through the interplanetary spaces by the existence of 'a thin ethereal medium,' incapable of perceptibly retarding the motion of the planets.

At first sight, it may seem strange that we should speak of the *acceleration* of the comet as being caused by the *retarding* influence of such a medium as has been conceived to occupy the interplanetary spaces. Yet it is strictly the case that, if a planet or comet be continually checked in its onward course, its velocity will continually grow greater and greater. For instance, if our earth were so checked, it would move in a spiral which would gradually bring its orbit to that of Venus, by which time its motion would be as rapid as that of Venus (which moves one-third faster than the earth); then it would continue revolving in a spiral till it reached the orbit of Mercury, when it would be moving as fast as this the swiftest of all the planets. And so the earth would continue to approach the sun with continually increasing velocity.

Returning to Encke's comet, we have to notice yet another important discovery which was effected by its means. The comet passed so near to Mercury in 1835 as to enable astronomers to form a much more satisfactory estimate of this planet's mass than had hitherto been obtained. It was found that the mass of Mercury had been largely over-estimated.

No very long interval passed after the discovery of Encke's comet before another comet of short period was detected. M. Pons, who had discovered Encke's comet, it will be remembered, in 1818, observed a faint nebulous object on June 12, 1819. This object turned out to be a comet; and in this case, as in the former, Encke calculated the stranger's orbit and period. He found that it moves in an ellipse which extends slightly beyond the orbit of Jupiter, and that it has a period of about five and a half years. This object was not seen again, however, until the year 1858, when M. Winnecke discovered it, and at first supposed it to be a new comet.

Calculation soon showed the identity of the two objects, and confirmed the results which had been obtained by Encke in 1819.

The next comet of short period was discovered by M. Biela in 1826. Perhaps nothing in the whole history of cometic observation is more surprising than what has been recorded of this singular object. We must premise that the comet had been seen in March 1772, and again in November 1805. But it was not until its rediscovery in 1826 that its orbit and period were computed. An ellipse of moderate eccentricity, extending beyond the orbit of Jupiter, was assigned as the comet's orbit—the period of revolution being about six and a half years. The orbit was found to pass within about twenty thousand miles of the earth's orbit; and at the first return of the comet (in 1832), some alarm was experienced lest the near approach of the two bodies should lead to mischief of some sort. The comet returned again in 1839 and 1845. It was at the last-mentioned return that a singular phenomenon occurred, which is nearly unique in the history of comets. On December 19, 1845, Hind noticed a certain protuberance on the comet's northern edge. Ten days later, observers in North America noticed that the comet had separated into two distinct comets, similar in form, and each having a nucleus, a coma, and a tail. European observers did not recognise the bi-partition of the comet until the middle of January 1846. The new and smaller comet appears to have sprung into existence from the protuberance observed by Hind, since this object moved towards the north of the other. After a while, the new comet became the brighter, but, shortly after, it resumed its original relative brilliancy. Lieutenant Maury noticed, on one occasion, a faint 'bridge-like connection' between the two comets. The distance between them gradually increased, until first the new comet, and then the old one, had passed out of view.

In 1852, Biela's comet was again seen, and the Padre Secchi, at Rome, detected a faint comet preceding it. If, as is probable, this faint comet is the companion, we may assume that the two bodies are permanently separated.

At the two next returns the comet was not seen, and much interest was felt by astronomers respecting the anticipated return in January 1866. It was searched for systematically at the principal European observatories. In fact, so closely did Father Secchi examine the calculated track of the comet that he detected several new nebulæ in that region. But the comet itself was not found. Astronomers are unable to assign any satisfactory reasons for its disappearance. It is known to have traversed the zone of the November meteors where that zone is richest—our readers will remember the display of shooting-stars in 1866—and Sir J. Herschel surmises that it may have been destroyed in the encounter. Possibly this may be the true solution of the difficulty; or, it may be that the comet was merely dispersed for a while during the passage of the meteor-zone, and may yet gather itself together and become visible to astronomers.[1]

We pass over three or four comets belonging to this class which present no special features of interest, to come to an object which has recently been rediscovered, and will continue visible (in good telescopes) for several weeks. On February 26, 1846, M. Brorsen discovered a telescopic comet, whose motions soon showed it to belong to the class of objects we are now dealing with. It was found to have an orbit of moderate eccentricity, extending just beyond Jupiter's orbit, and a period of about five and a half years. It was not seen at its next return to perihelion; but was rediscovered by M. Bruhns on March 18, 1857. In 1862, it again escaped undetected; but at its present return, it has been rediscovered (by three observers simultaneously), and it is now being carefully tracked across the northern skies.

In all, there have been recognised thirteen comets of short period—that is, having periods of less than seven years. Amongst these are included several which have only been seen once, and some which are known to have been subjected to such disturbance as no longer to travel in orbits of short period. Of these thirteen comets, no less than *ten* have the aphelia of their orbits just beyond the orbit of the planet Jupiter; *two* have their aphelia just within Jupiter's orbit; and Encke's comet alone has its aphelion at a safe distance from that orbit. It appears to us that the peculiarity thus exhibited is not without meaning. Remembering the history of Lexell's comet, we seem to find a satisfactory explanation of the peculiarity. We have seen how Lexell's comet was first introduced into the system of short-period comets by the giant planet Jupiter, and then summarily dismissed. So long as the comet remained within that system, the aphelion of its orbit lay just beyond the orbit of Jupiter, *and this would be the case with any comet introduced in a similar manner*. But for the coincidence which led to its expulsion, Lexell's comet would have continued to revolve as a short-period comet. It seems also clear that in the course of many ages its period and orbit would have grown gradually smaller, through the operation of the same cause (whatever that may be) which is now reducing the period and orbit of Encke's comet. At length it must have attained a path safe within the orbit of the great disturbing planet. In the list of short-period comets, then, we seem to see illustrations of the successive stages through which Lexell's comet would have passed in attaining the sort of orbit in which Encke's comet is now moving. And it seems permissible to assume that *all* the short-period comets have been introduced to their present position within the solar system by the same cause which led to the temporary appearance of Lexell's comet as a comet of short period—that is, by the attractive energy of the planet Jupiter.

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[1] The return of this comet in 1872 was eagerly looked for by astronomers. But the comet was not seen. On November 27, 1872, there was a fine display of meteors, as the earth passed through the comet's track, and afterwards a cometic object was seen in the direction towards which the meteors had been travelling. But this was not Biela's comet, which, indeed, must have passed that place nearly twelve weeks earlier. Indeed, some doubt exists whether the object was travelling in the track either of the comet or of the meteors. The comet was also sought for in vain at its (presumably) next return in 1878.

Chambers's Journal, July 1868.

THE GULF STREAM.

MAJOR RENNELL was the first, I believe, to whom we owe the comparison of ocean-currents to rivers. He spoke of them as ocean-rivers, and pointed out how enormously their dimensions exceed those of such streams even as the Amazon and the Mississippi. Some of the ocean-currents are from 50 to 250 miles in breadth, and flow more swiftly than the largest navigable rivers. The banks and bottom of these currents are not land, but water; and so deep are the currents that they are turned aside by shoals and banks whose tops are '40, 50, or even 100 fathoms beneath the surface of the ocean.' The outlines of ocean-currents are sharply defined, insomuch that 'often,' says Captain Maury, 'one half of a vessel may be seen floating in the current, while the other half is in common water of the sea.' The border-line of the Gulf Stream can be traced by the eye. Yet more remarkable is the distinction between the moving water and that which is at rest, when large masses of sea-weed carried along by the former enable one to recognise the rapidity with which it moves.

Of all the ocean-currents the most important, perhaps, in its bearing on the destinies of men and nations, is the great Gulf Stream. I propose to examine the course and habitudes of this current, and then to inquire a little into the vexed question of its cause.

Major Rennell traced the Gulf Stream from a supposed source in the Indian and Southern Oceans. Modern geographers and physicists prefer to look for the rise of the current somewhere near the Cape of Good Hope. 'The commencement and first impulse of the mighty Gulf Stream is to be sought,' writes Humboldt, 'southward of the Cape of Good Hope.' It appears to me, however, that the true source of the great stream is to be looked for in the equatorial zone of the Atlantic. When we come to inquire into the cause or causes which give birth to the Gulf Stream, we are led, as I imagine, to this region rather than to any other (though, perhaps, in a stream which forms part of a continuous system of circulation, we can hardly speak of any one portion as the source); I shall therefore trace the stream, and the system to which it belongs, from the great equatorial waters which move, as Columbus was the first to discover, 'with the heavens (*las aguas van con los cielos*), that is, from east to west, following in this the apparent motions of the sun, moon, and stars.'

The map of the Atlantic Ocean on the opposite page is constructed upon one of those forms of isographic projection described in my 'Essays on Astronomy.' It is important, in dealing with the subject of currents, that the question of area should be considered, and, therefore, that our illustrative charts should represent such areas correctly. This Mercator's charts are far from doing. The portion of the Atlantic Ocean between England and the United States of America is unduly magnified, and still more is this the case with the portion between Sweden and Greenland. On the other hand the portion between Africa and the Gulf of Mexico is unduly diminished. Thus it is scarcely possible to form from such charts just notions of the actual character of the oceanic circulation whereof the Gulf Stream forms a part. (Compare the charts illustrating the 'Essay on the Climate of Great Britain.')

We see in our map[1] that there is a great equatorial stream extending in its eastern portion far to the south of the equator, but passing to the north also even here, and still further to the north between the coasts of Africa and South America. Near here the great equatorial current divides into two portions. One passes southward and then returns towards the east, according to some authorities, but, according to others, continues its course southward until it is lost in the Antarctic Ocean. We shall follow the northern bifurcation, however. The course of this portion of the Atlantic current system has been far more exactly traced out. Taking a north-westerly course, the great current pours itself against the barrier formed by the Leeward and Windward Islands. Passing between these islands, it sweeps around the shores of the Gulf of Mexico, a portion, however, of its volume passing probably outside the West Indian Islands, to rejoin the other outside the promontory of Florida. At this point the stream has become, probably, somewhat diminished in volume, but, being still more diminished in breadth, it flows as a deep, strong, and swift stream, known among sailors as 'The Narrows of Bemini.' From hence the stream, now become the true Gulf Stream, grows gradually wider, less deep, and less swift. Off Hatteras it is already twice as broad as in the Florida Straits, and as it stretches with a wide easterly sweep across the Atlantic towards the shores of Ireland and the Hebrides, the current not only reassumes something of its original extent of surface, but again bifurcates; a wide but somewhat sluggish stream is sent southward towards the shores of north-western Africa, to rejoin the equatorial stream. The main portion of the current, however, passes with a north-easterly course up the Atlantic valley, between Iceland and Sweden to the Polar seas. It seems uncertain whether Rennell's current, which passes around the Bay of Biscay, and the current which streams southward past the shores of Spain, are forks of the Gulf Stream. They are usually represented in maps as independent currents, and in Captain Maury's large map of the Gulf Stream the great southern bifurcation already mentioned is represented as a current impinging upon the flank of the stream which flows past Spain and north-western Africa. Yet, if these streams have not their source in the Gulf Stream, it will be found no easy problem to assign their origin; and I cannot but think that the Biscay and Guinea currents, as well as the current which flows into the Mediterranean through the Straits of Gibraltar, are as truly bifurcations of the Gulf Stream as the current which laves the shores of Ireland and Sweden.

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[1] For the sake of completeness, and also that the present essay may fairly represent my views when it was written, I leave the account of the map and of the course of the Gulf Stream unchanged here. By comparing this essay with the following, it will be noticed that only very few passages are repeated in substance.

There will be noticed also in the map three return streams, one flowing southward outside Iceland, another sweeping round the eastern shores of Greenland, and the third flowing through Baffin's Bay and Davis's Straits. The two last unite south of Davis's Straits, and flow on together to meet the first stream outside Newfoundland, whence the three flow as a single current past the shores of the United States. It is generally assumed, and in all probability justly, that these three streams are derived from the Gulf Stream, and are different branches of its returning waters.

Between the single return-stream which laves the shores of the United States and the Gulf Stream there is an unshaded space in the map. It is not to be inferred, however, that this space represents still (or rather unflowing) water. On the contrary, it is the 'debatable ground' between the opposite currents. In spring the whole of this space is occupied by the southward flowing waters of the cold return-current. In autumn the whole of the space is occupied with the waters of the Gulf Stream. Backwards and forwards over this space the rival currents are continually swaying, the period of an oscillation being one year.

In the widest part of the Atlantic Ocean—that, namely, which extends between the most westerly part of Africa and the West Indies—there is a wide expanse of waters unmoved by the flux or reflux of currents. Surrounded on every side by the circulating waters of the Central Atlantic current-system, this region remains undisturbed save by winds and the tidal wave. Accordingly its surface is covered with different forms of marine vegetation. My readers will doubtless remember the interest which the Great Sargasso Sea excited in the mind of Christopher Columbus. Oviedo termed this region the 'seaweed meadow.' 'A host of small marine animals,' says Humboldt, 'inhabit this ever-verdant mass of *Fucus natans*, one of the most widely-diffused of the social plants of the ocean, constantly drifted hither and thither by the tepid winds that blow across its surface.'

In the South Atlantic there is a smaller and somewhat more sharply-defined Sargasso, covered chiefly with rockweed and drift. A weedy space occurs also about the Falkland Islands, but is probably not a true Sargasso. Maury considers that the seaweed reported there probably comes from the Straits of Magellan, where it grows so thickly that steamers find great difficulty in making their way through it; for it so cumbers their paddles as to make frequent stoppages necessary.

Such is the distribution of the surface of the Atlantic Ocean. But now the question will at once suggest itself: Is the complete system of oceanic circulation exhibited on the surface? It seems now quite certain that this question must be answered in the negative. We might, indeed, at once point to the existence of the important current which laves the shores of the United States as an answer to the question; for where can all this water find an outlet? It does not pass the Peninsula of Florida as a current; it does not cross the Gulf Stream; where, then, can it go but underneath the ocean's surface? But we have positive evidence of the existence of under-currents.

In the first place it is found that in deep-sea soundings in many parts of the ocean, far more line may be paid out without any sign of the bottom being reached than the depth of the ocean in those parts would account for. In places where it has been proved by other methods than ordinary sounding that the depth is not more than three miles, no less than ten miles of line have been paid out, being carried out so strongly that the slightest check in the paying-out apparatus has sufficed to break the sounding-line.

In the second place, it has been found possible to determine the depth at which a submarine current is flowing, and the direction in which it flows. Thus Lieuts. Walsh and Lee, in the American service, having loaded a block of wood to sinking, and let it down to different depths, had repeatedly the satisfaction of seeing the work of under-currents. 'It was wonderful, indeed,' they write, 'to see the *barrega*' (a float attached to the upper end of the line) 'moving off, against wind, sea, and surface current, at the rate of over one knot an hour, as was generally the case, and on one occasion, as much as one and three-quarter knots. The men in the boat could not repress exclamations of surprise, for it really appeared as if some monster of the deep had hold of the weight below, and was walking off with it.'

Lastly, we may mention that Captain Wilkes, of the United States Exploring Expedition, established the existence of a cold under-current no less than two hundred miles broad at the equator.

We may assume, then, that a complete system of circulation, vertical as well as horizontal, exists throughout the whole of the waters contained within the great Atlantic valley.

Where are we to look for the origin of this vast series of movements? The actual 'work done' in the Atlantic Ocean is so enormous—in other words, the transfer of such large volumes of water represents so enormous a *force*, that we might well expect to be able at once to assign the motive-power of this great machinery. For it would seem that the giant which works such wonders could not readily hide himself from our recognition.

It has not been found, however, that the solution of the problem has been so simple as was to have been anticipated.

Passing over the earlier guesses which marked the Gulf Stream as the offspring of the Mississippi River, of the sun's motion in the ecliptic (a mysterious interpretation of the phenomena), and of the tidal wave, we may remark that but two explanations of the Atlantic currents seem to merit discussion.

Sir John Herschel is the principal exponent of the first theory, which assigns to the trade-winds the principal—almost the sole—agency in the generation of the Atlantic current-system. He refuses indeed, to look on the subject as one of any doubt or difficulty. 'The dynamics of the Gulf Stream have of late,' he writes, 'been made a subject of much (we cannot but think misplaced) wonder, as if there could be any possible ground for doubting that it owes its origin *entirely* to the trade-winds.' 'If there were no atmosphere, there would be no Gulf Stream, or any other considerable oceanic current (as distinguished from a mere surface-drift) whatever.' He presents his solution somewhat as follows:—The trade-winds are an actually existent cause for an easterly motion in the tropical seas; we cannot ignore their action; we know, also, that when the trade-winds arrive at the equator, they have lost their easterly momentum; and we know, therefore, that that momentum must have been imparted to the surface of the water (for where else can it have gone?); hence there arises the great easterly movement which generates the whole system of circulation.

The second view, which attributes oceanic circulation to differences of temperature and saltness in different parts of the ocean, is supported by Humboldt and others, but is taken up most unflinchingly by Captain Maury, who assigns it as practically the sole cause of all oceanic circulation. 'The Gulf Stream,' he writes, 'as well as all the *constant* currents of the sea, is due mainly to this cause. Such differences are inconsistent with aqueous equilibrium, and to maintain this equilibrium the great currents are set in motion. The agents which derange equilibrium in the waters of the sea, by altering specific gravity, reach from the equator to the poles, and in their operations they are as ceaseless as heat and cold; consequently, they call for a system of perpetual currents to undo their perpetual work.' 'Other causes *help* to cause currents,' he says, 'but the currents created by them are *ephemeral*.'

Here we have what is 'a very pretty quarrel as it stands.' Each of the disputants points to causes of acknowledged importance, and also (whether efficient or not in the particular matter under question) of acknowledged general efficiency. Each has much to say in favour of his own view, and each considers his antagonist's agent as utterly insufficient for the work ascribed to it. Each has heard his opponent's arguments, and reiterates his own statement. Nor can it be said that the opponents are unequally matched; for, if we must place Sir John Herschel far before Maury as a mathematician and physicist, and if we must undoubtedly look upon the former as the more practised reasoner, yet we must remember, in turn, the special attention which Captain Maury has given to the subject under discussion, and the practical acquaintance with it which his experience as a seaman has given to him.

Let us briefly state the arguments adduced by Herschel against Maury's view, and by Maury against Herschel's.

Sir John Herschel asserts that, inasmuch as the sun's heat warms the *surface* of the ocean most intensely, so

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that the water of least specific gravity is already uppermost, there can be no tendency to motion. For the heated water cannot *descend*, being buoyant; nor *ascend*, being uppermost; nor move *laterally*, having no impulse to motion of that sort, and being only able to move laterally 'by reason of a general declivity of surface, the dilated portion occupying a higher level.' He then applies to this declivity the test of quantitative analysis. Taking a column of water at the equator having at the base a temperature of 39° (at which temperature fresh water attains its greatest density, and which is also the temperature of water 7,200 feet beneath the surface at the equator), while its top has a temperature of 84° (the warmth of equatorial surface-water), he finds that such a column is 10 feet higher than a similar column in latitude 56°, where 39° is the surface temperature. And since from the equator to latitude 56° the distance is 3,360 geographical miles, we have a declivity of barely one-twenty-eighth of an inch per geographical, or one-thirty-second of an inch per statute mile. Such a declivity is utterly insufficient to account for the existence of a strong current from the equator towards the tropics; while, so far from giving any account of the motion of the equatorial current from east to west, it would tend to form a north-easterly current.

This seems strongly opposed to Maury's view, and I do not find that he does much to get over the force of Herschel's reasoning. He points out, indeed, that sea-water does not attain its greatest density at a temperature of 39°, but some 12° or 14° lower. This, however, does not affect Herschel's argument. If he had taken a column whose base had a temperature of 25° instead of 39°, he would have had to extend, also, the range of the water-slope in latitude; and, in fact, he would have obtained a yet smaller declivity in this way than that actually deduced by him. Maury does not seem to have noticed the really weak point in Herschel's argument. I shall presently show where this seems to me to lie.

But if Maury fails in efficiently defending his own views, he certainly is sufficiently effective in his attack upon Sir John Herschel's.

He asks, in the first place, the pertinent question—'How can the north-easterly trade-winds, which blow only 240 days out of of 365, cause the equatorial current to flow all through the year towards the north-west without varying its velocity either to the force or to the prevalence of the trade-winds?' 'That the winds do make currents in the sea, no one,' he says, 'will have the hardihood to deny; but currents that are born of the winds are as unstable as the winds; uncertain as to time, place, and direction, they are sporadic and ephemeral.'

He then points to a fact which 'militates strongly against the vast current-begetting power that has been given by theory to the gentle trade-winds. In both oceans, the Sargasso seas lie partly within the trade-wind region; but in neither do these winds give rise to any current. The weeds are partly out of water, and the wind has therefore more power upon them than it has upon the water itself; they tail to the wind. And if the supreme power over the currents of the sea reside in the winds, as Herschel would have it, then of all places in the trade-wind region we should here have the strongest currents. Had there been currents here, these weeds would have been borne away long ago; but so far from it, we know that they have been in the Sargasso Sea of the Atlantic since the voyage of Columbus.'

In another argument, Maury certainly falls into an error. He says, How can the north-easterly winds cause the Gulf Stream to flow *towards* the north-east? But, as he himself points out, the trade-winds do not blow over the Gulf Stream proper, and there can be no doubt that, if the trade-winds sufficed to keep up a continual equatorial current, finding a passage towards the north after encountering the barrier opposed by the American continent, this resulting northerly current would assume a north-easterly course, for the very same reason that the air-currents flowing from the equator towards the north pole become south-westerly or counter trade-winds. But he seems justified in asking how it is possible that the impulse imparted by the gentle trade-winds to the equatorial current could suffice to generate a stream which eventually travels far towards the north pole, if it do not even circle completely around Greenland. 'When we inject water into a pool,' he says, 'be the force never so great, the jet is soon overcome, broken up, and made to disappear. In this illustration, the Gulf Stream may be likened to the jet, and the Atlantic to the pool. We remember to have observed, as children, how soon the mill-tail loses its current in the pool below; or we may now see at any time, and on a larger scale, how soon the Niagara, current and all, is swallowed up in the lake below.'

Franklin, who was the originator of the theory supported by Herschel, had unnecessarily introduced the supposition that the trade-winds maintain a 'head of water' in the Gulf of Mexico, and that the Gulf Stream flows downwards like a river from this 'head,' as a fountain or source. Maury rightly attacks this view, which is undoubtedly a mistaken one; but in doing so, he falls into an error which exhibits his weakness in the treatment of hydrodynamical problems. He points out that, inasmuch as the Gulf Stream grows wider as it crosses the Altantic, it necessarily grows shallower, so that the water-bed in which the stream flows has a higher level under the shallow than under the deep part of the current, and therefore, says Maury, '*the current runs up hill.*' Herschel terms this a strange perversion of language, but perhaps it would be more correct to speak of it as a strange blunder. The stream could, of course, only be said to run up hill if *its surface* were seeking a higher level, which does not and cannot happen. That the spreading out of the water of the current, so as to form a wider and shallower stream, does not correspond to an upward flow, is evident from this, that it happens often with rivers, which no one will suspect of *running up hill.*

Herschel does not find an answer to the main objections urged by Maury against the trade-wind theory. Content with urging an apparently unanswerable objection against his opponent's view, he leaves his own to take care of itself.

In forming an opinion respecting the two theories, one is struck with the immense superiority in the *power* of Maury's agent. For, if we consider, we shall see that almost *the whole of the sun's action upon the ocean* goes to produce those variations in temperature and saltness in which Maury sees the origin of the current-system; but a very moderate portion of the sun's action is called into play in the production of the trade-winds. Now it is very doubtful whether any large proportion even of the force expended in producing the trade-winds ever acts on the water. For we know that the north-easterly and south-easterly air-currents of the northern and southern hemispheres do not wholly merge into northern and southern currents meeting point-blank near the equator, as Herschel's theory seems to imply. On the contrary, there is a wide zone of calms at the equator, and the two systems of trade-winds appear to pass upwards above the calm air, without parting with the whole of their easterly motion. When once they begin to travel polewards, they lose their easterly motion in the same way that they acquired it—that is, through the effects of the earth's rotation. And whatever portion is lost in this way—which, for aught we know, may be a very considerable portion — cannot be taken into account as available to generate the easterly equatorial current.

And now let us consider for a moment the relation which holds between cause and effect in the case supposed by Herschel. We have more than a fourth part of the Atlantic Ocean in a state of perpetual motion, and it is assumed that the air immediately above the ocean is responsible for this circulation. Now even if we suppose that the whole of the *vis viva* in the aërial circulation is imparted to the waters, and neglect all consideration of the fact that for a large portion of the year the winds do not act in the manner available for the production of the currents we are considering, yet, even then, I apprehend that we shall find the *vis viva* of the aërial very far below that of the aqueous circulation. The volume of moving water is, of course, far less than that of the moving air, and the mean velocity of the water-currents is less than that of the air-currents; but, on the other hand, the specific gravity of water is some 830 or 840 times greater than that of air, and this difference far more than counterbalances the others.

But now, when we come to consider the forces called into action in producing changes of temperature, &c., we no longer find such a disproportion between cause and effect. The sun's action on the equatorial and tropical regions of the Atlantic not only produces a great change in the density of the water, but also raises immense masses by evaporation. Now the buoyancy due to the increase of temperature is partly diminished through increase of saltness; still it is an important motive force. A large portion of the evaporated water is also precipitated over the equatorial regions in the form of rain; yet that a very large portion is carried away from equatorial and tropical to temperate zones is beyond dispute.

But now, how are we to get over the arguments by which Herschel seeks to show that the buoyant water will not rapidly move off, and that the effect of evaporation is merely to produce opposing inrushes of water which destroy each other's effect? Easily, I take it, if we remember that the buoyancy of the water *does* produce a surface-flow from the equator, however slight, and that this is sufficient to destroy the balance of forces which might otherwise make it doubtful whether the place of the evaporated water would be supplied from below or from above. I apprehend that there is a continual under-flow of cooler water, rushing in towards the equator on both sides, to supply the place of the water evaporated by the sun's heat. Now there can be no question that under-currents arriving in this manner, whether from the north or from the south, would acquire a strong westerly motion (just as the trade-winds do). Thus they would generate *from below* the great equatorial westerly current. In this up-flow of cool currents having a strong westerly motion, I find the mainspring of the series of motions. The water thus pouring in towards the equator is withdrawn from beneath the temperate and arctic zones, so that room is continually being made for that north-easterly surface-stream which is the necessary consequence of the continual flow of the great western equatorial current against the barrier formed by the American continent.

It would require much more space than I have at my disposal to deal at length with the subject of my paper. I therefore conclude by referring my readers to Maury's interesting work on the 'Physical Geography of the Sea,' with the remark that his views seem to me only to require the mainspring or starting force towards the west which I have ventured to suggest, to supply a complete, efficient, and natural explanation of the whole series of phenomena presented by the great ocean-currents.

The Student for July 1868.

OCEANIC CIRCULATION.

THERE are some questions, seemingly innocent enough, which yet appear fated to rouse to unusual warmth all who take part in their discussion. One cannot, for instance, find anything obviously tending to warmth of temper in the telescopic study of a planet; yet the elder Cassini was moved to passionate invective by certain observations of Mars not perfectly according with his own; and Sir W. Herschel, usually so philosophic, was roused by Schröter's recognition of mountains in

Venus to deliver himself of a criticism justly described by Arago as 'fort vive, et, en apparence du moins, quelque peu passionnée.' The question, again, whether the 'Eozoon Canadense' is a true 'Rhizopod,' though not altogether removed from the region of hard words, might appear to be unlikely to excite warlike emotions; yet there has been some very pretty fighting over it. The solar corona has in like manner given occasion for rather strong writing; and if, on the one hand, the supporters of a lately abandoned theory said of their opponents that 'they made themselves ridiculous,' these, in their turn, at times used a tone reminding one of the scholar who said of a rival, 'May God confound him for his theory of the Irregular Verbs:' yet the corona seems at a first view rather calculated to produce a sedative effect than to excite unphilosophic wrath. The subject of oceanic circulation would appear to belong to the class of questions here considered.

The very name of the Gulf Stream is to some physical geographers as a red cloth is to a bull. Even Sir John Herschel, usually placidity itself, was moved when he spoke on this point. But though he and Maury grew warm enough in its discussion, their warmth was ice-cold compared with the fire of more recent disputants. We have before us the latest contribution to the subject, a rather ponderous essay in one of our leading quarterlies; and herein we find pleasing references to the 'stupidities' of one set of opponents, the 'shallow nonsense' of a second, 'the wrong-headedness' of a third, with other similar amenities. More than once during the progress of this controversy the gentle public has been reminded of Bret Harte's remarks

> . . . about the row
> That broke up the Society upon the Stanislow;

and has been inclined to urge, with 'Truthful James,' that they

> Hold it is not decent for a scientific gent
> To say another is an ass,—at least to all intent;
> Nor should the individual who happens to be meant
> Reply by heaving rocks at him to any great extent.

The controversy has not, indeed, reached this last stage of development, and we trust it never will; but it has gone so near to it as to suggest that the disputants have wished to demonstrate, by example, the justice of Darwin's theory about the human 'snarling muscles.'[1]

I propose to inquire into the subject which has been thus warmly discussed, trusting not to be myself inveigled by it into any warmth of expression. Indeed, but for the fate of others, I should feel no anxiety on this point, though I have myself a favourite theory to uphold respecting one branch of the subject. As it is, I share something of the feeling of the Red Cross Knight when he was approaching 'Foul Error's den,' and his monitress said to him, 'The perils of this place I better wot than thou; therefore I rede, Beware.' I am not without hope, however, that I may be able to keep my snarling muscles quiescent.

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[1] 'He who rejects with scorn the belief that the shape of his own canine teeth, and their occasional great development in other men, are due to our early progenitors having been provided with these formidable weapons, will probably reveal, by sneering, the line of his own descent. For though he no longer intends, nor has the power, to use these teeth as weapons, he will unconsciously retract his "snarling muscles" (thus named by Sir Charles Bell), so as to expose them ready for action, like a dog prepared to fight.'—Darwin's *Descent of Man*, vol. i. p. 176. We may mention, by the way, that an instance has recently occurred, in which the human teeth were used to some purpose against one of the recognised masters in the art of biting. A man, proceeding in company with several others through a wood, was attacked by a hyena (usually one of the most cowardly of beasts). His companions fled, and having no weapon he was reduced to the necessity of showing tooth for tooth, and, taking a good grip of the hyena's nose, he compelled that gentleman to howl with anguish. On this, the man's companions returned and presently beat the hyena to death.

I shall direct attention chiefly to the Atlantic currents, as being those whose real direction and extent are best known, and those, moreover, whose characteristics are most important to European nations.

Let us begin with the surface currents, and though the system of surface circulation can scarcely be said to have a real beginning, let us start with the great equatorial currents which flow westwards from the Gulf of Guinea,[1] or more correctly from the Bight of Biafra. We speak of the westwardly equatorial *currents*, because not unfrequently there is an equatorial eastward current running between two much more important *tropical* westward currents. Yet ordinarily there is one great westward current running in an unbroken stream from equatorial Africa to the shores of Brazil, and even when this great current is divided into two by an eastward current, this last is only to be regarded as a sort of 'backwater.' The water moving westwards is relatively cold, more especially on the African side of the Atlantic.

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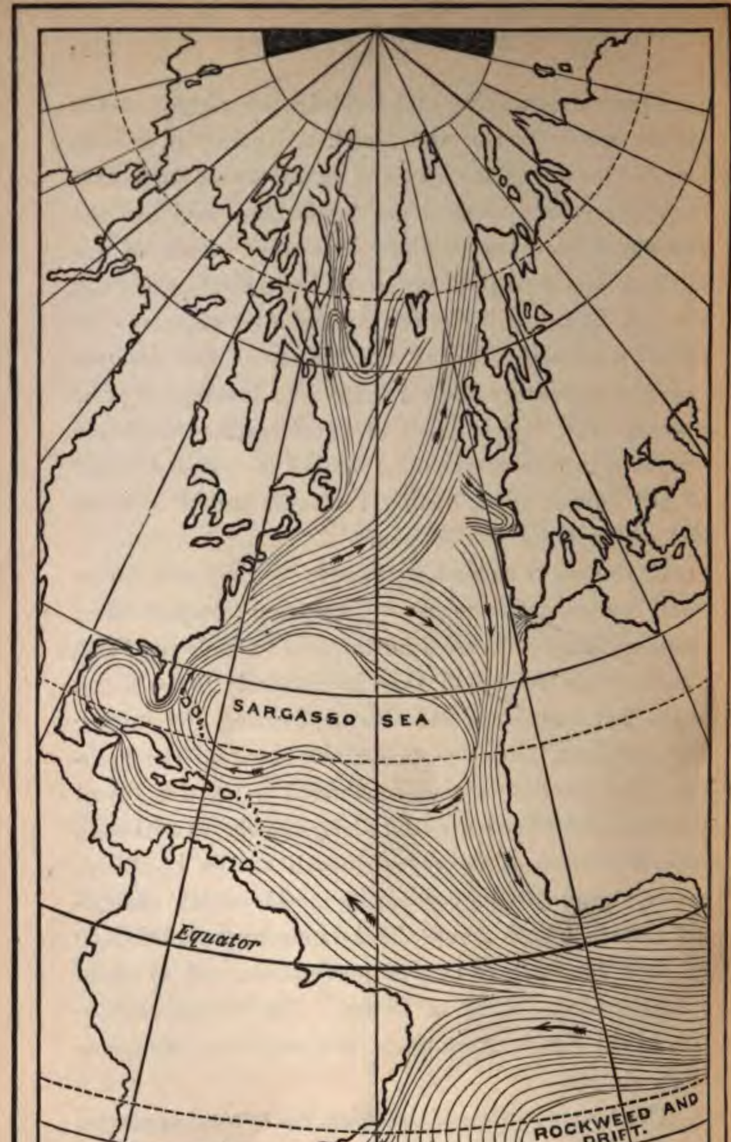
[1] Along the *shores* of the Gulf of Guinea there flows an easterly current, several degrees warmer than the equatorial current.

The accompanying map exhibits the nature of the surface circulation of the North Atlantic. It is constructed on one of the forms of equal-surface projection described in my 'Essays on Astronomy,' and has the advantage over the ordinary Mercator's charts of exhibiting the true dimensions of the various currents. I would, however, invite the student who wishes to familiarise himself with the true nature of the Atlantic currents to construct other maps: for instance, a polar map on the first method of equal-surface projection described in that essay, and a map of the whole Atlantic on the second plan, taking the meridian 40° west of Greenwich as the central one.

Of the water carried westwards by the great equatorial movement, the most important portion after reaching Brazil is carried northwards towards the West Indies. The reason of this is obviously to be found in the fact that Cape San Roque, forming the jutting angle of Brazil, lies several degrees south of the equator. The portion carried southward forms the Brazil Current, and after travelling along the shores of South America almost as far as the mouth of the La Plata, acquires gradually an eastwardly motion which eventually carries it back across the Atlantic towards the Cape of Good Hope, there to pass northwards, and so again to traverse the Bight of Biafra. The surface-circulation in the South Atlantic is thus seen to be comparatively simple.

The larger portion of the equatorial current is carried less quickly northward, because the northern shore-line


SARGASSO SEA
Equator
ROCKWEED AND DRIFT.

of Brazil and Guiana is inclined at a much smaller angle than the south-eastern to the westerly course of the great equatorial currents. Thus the water which is carried towards the West Indies has time to acquire under the tropical sun a much higher temperature than it had possessed when traversing the Gulf of Guinea. It is divided into two parts by the *quasi*-barrier which the West Indian Islands (or rather the semi-submerged mountains of which they form the crests) oppose to its progress. A comparatively small portion finds its way into the Caribbean Sea, and making the circuit of the Gulf of Mexico, passes out eastwards round the peninsula of Florida. We may fairly assume that this portion *is* comparatively small; simply because this true gulf stream, passing between Cuba and Florida on an eastern course, would continue so to move for at least some considerable distance, were it not in some way deflected. But it actually turns almost due northwards after passing through the Bahama Sea, traversing the Bemini Narrows on this course, and so onwards towards Hatteras. This would seem to imply that the true Gulf Stream is pressed northwards by the arrival of a much larger body of water which has travelled outside the West Indies. It is true that the diversion of the Gulf Stream northwards may be really caused by the great Bahama Bank. But this would equally establish our position; for if the Bahama Bank is thus effective in diverting the whole of this now swiftly moving current, the Windward Isles may be assumed to be correspondingly effective in diverting the greater portion of the sluggish equatorial current. Moreover, if we remember how shoals commonly take their origin, we may consider that the very existence of the Bahama Bank is probably due to the former encounter of the two important branches of the equatorial current—the part which had circled the Gulf of Mexico and the part which had travelled outside the West Indies. Thus, the northerly course finally taken by the Gulf Stream implies that the latter portion had prevailed over the former, and therefore that it is the most considerable portion. I must mention, however, that the Edinburgh Reviewer holds the part which enters the Caribbean Sea to be the larger.

Be this as it may, the Gulf Stream proper has acquired, during its circuit, characteristics perfectly distinct from those which it had had when entering the Caribbean Sea, or from those possessed by the remaining portion when approaching the Bahamas. In the first place, having traversed a much longer course under the same intense tropical heat, the Gulf Stream has become much warmer than the outer stream. In the second place (probably from having traversed the outlets of the Mississippi, and so carrying with it the finely-divided matter brought down by that river), the Gulf Stream has acquired a peculiar blue colour, somewhat resembling that recognised in most of the Swiss lakes.[1]

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[1] This explanation of the colour of the Gulf Stream seems the best that has hitherto been offered. The Edinburgh Reviewer thus states the matter:—'The remarkable blueness which distinguishes the water of the Gulf Stream from the oceanic water through which it flows may be due to its retaining in suspension the finest of the sedimentary particles brought down by that river, the coarser having been de-

Thirdly, its course having carried it into narrow channels, it has acquired a relatively rapid rate of outflow, insomuch that the surface flow of the current on its outward passage through the Narrows of Bemini, takes place at the rate of from 2½ to 4 miles per hour. Its width here is at the surface not more than about 25 miles, its maximum depth rather more than a quarter of a mile (about two-fifths of the channel's maximum depth), and its mean rate of flow probably about 50 miles per day.

I shall not follow the Edinburgh Reviewer in considering the details of the progress of the Gulf Stream from the Narrows of Bemini to Cape Hatteras, because, though in themselves of the utmost interest and importance, these details throw no special light on the general subject of oceanic circulation. Suffice it that as far as Hatteras the Gulf Stream remains distinctly recognisable, and that even off Sandy Hook (New York) its surface temperature is little reduced, and its velocity still amounts to about one mile per hour. Off Nantucket the breadth of the current is about 410 miles,

posited near its (the river's) mouth; just as the intense blueness of the waters of Lake Geneva depends on its retention of the finest sedimentary particles brought down by the Rhone in the upper part of its course, while that of the waters of the Mediterranean is due to its pervasion by the like particles brought down by the river Rhone and other rivers, which discharge themselves into its western basin, and by the Nile into its eastern.' It will be remembered that Prof. Tyndall, by researches carried on during the return of the *Urgent* from the eclipse expedition of 1870, was enabled to throw considerable light on the cause of the colour and shades of colour in water of greater or less depth. See also Dr. Carpenter's 'Report of Researches in the Mediterranean,' in the 'Proceedings of the Royal Society,' vol. xix. p. 200.

its winter surface temperature only 10° below that which it had in the Florida Channel, and its rate of flow still nearly one mile per hour. It has at this part of its course acquired a good deal of easting, a circumstance which must (unquestionably, we conceive) be ascribed to the fact that it brings from low latitudes the more rapid easterly rotation movement of the earth. The same would, of course, apply to the less characteristic but larger current which has arrived at the same latitudes *without* circuiting the Gulf of Mexico.

Now here we approach a critical part of our subject. It is admitted by all that off Newfoundland the Gulf Stream loses its special characteristics. As Dr. Hayes remarks, 'its strength diminishes; the air of a higher latitude brings its temperature down to that of the North Atlantic generally' (not, however, without raising the temperature of the North Atlantic to some extent); 'the water loses all its Gulf Stream character as to course, warmth, and flow' (and as to colour also); 'and it dies away into the sluggish Atlantic drift which sets from a westerly to an easterly direction.' It is not so generally noticed, but will scarcely, I suppose, be disputed, that the Gulf Stream water strengthens, and that appreciably, this sluggish Atlantic drift. Then it is reinforced by the portion which has travelled outside the West Indian Islands; and we may assume (without giving rise to objections) that the general prevalence of south-westerly winds will further strengthen the eastward motion of the combined mass. At any rate, let the causes be what they may (and presently we shall have a further cause to take into account), it is admitted by all physical geographers that a great, though slow current, or drift, *does* pass eastwards from the neighbourhood of Newfoundland. Moreover, it is admitted by all that the southern part of this current (which the Edinburgh Reviewer actually regards as identifiable with the Gulf Stream [1]) traverses the Atlantic until, nearing the Azores, it joins the southwardly Guinea current; while the northern part passes on a north-easterly course, which carries it between Britain and Iceland, between Sweden and Spitzbergen, onwards, even as far as the very neighbourhood of Nova Zembla. Lastly, it is admitted by all that, directly, or indirectly, this great north-easterly current causes the climate of Great Britain, and of the north-western parts of Europe generally, to be milder than that of North American regions in corresponding latitudes.

It might appear, then, that all these things being admitted, no question of any importance remains, so far as the actual facts of the oceanic surface-circulation are in question. We shall presently see that a question has arisen as to the *cause* of the observed facts; but as to their *nature* everything that seems worth discussing at all appears to be satisfactorily disposed of.

Let those readers who in their simplicity have adopted this notion hasten to dispossess themselves of it, by reading some remarks by Dr. Hayes, the American explorer, quoted with approval by the Edinburgh Reviewer. The latter having repeated from 'Lothair' 'a sneer at the shallow nonsense which has been talked about the Gulf Stream, and at the exaggerated estimates of its potency which have been put forward by men (as well as women) who ought to have known better' (these are the reviewer's words, not Mr. Disraeli's), proceeds as follows: 'As Dr. Hayes truly remarks, "Weather predictors without end have launched upon it their stupidities; meteorologists have deluged the world (*sic*) with their assumptions respecting it; theorists of all kinds have floated their notions upon it. One whirls it away into the arctic regions, and opens a passage to the pole with it; another compels it to give a climate to countries where otherwise there would be no climate worth mentioning; while still another spins it round the Atlantic Ocean, and its wide-spread arms close upon a stagnant sea. . . . Through means such as these mankind has come to look upon the Gulf Stream with a certain degree of awe. It is a 'breeder of storms'; the giver of heat; it might become the father of pestilence. Will it always continue to do its duty as hitherto? or will it start off suddenly with some new fancy, and pursuing some new course, upset the physical and moral status of the world?"'

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[1] He says that the great equatorial current is partly supplied 'by the return of a portion of the Gulf Stream.'

Now we have seen that the writer who thus endorses Dr. Hayes' diatribe, is among those who hold that a southern offset from the Gulf Stream circles round the Sargasso Sea to join the Guinea current. He says farther on that he 'entirely accords' with the opinion of Buchan, the meteorologist, that the north-easterly current above referred to 'produces an afflux of warmth brought to the British Isles by the water that laves our western coasts.' He proceeds: 'There is ample evidence that the cold of some parts of the north polar area is greatly mitigated by an afflux of water bringing with it the comparative warmth of temperate seas. It has long been known that cocoa-nuts, tropical seeds, trunks of tropical trees, timbers and spars of ships wrecked far to the south, and sometimes portions of their cargo, are found on the shores of the Western Hebrides, the Orkney, Shetland, and Faroe Islands, the north of Norway, and even Spitzbergen; and since their transport has taken place just in the course of the Gulf Stream if prolonged to the north-east, their arrival has been accepted almost without question as evidence of its agency. The evidence furnished by the surface temperature of that north-eastern portion of the Atlantic Ocean which intervenes between Iceland and the North Cape, and then stretches away to the eastward between Spitzbergen and Nova Zembla, seems at first sight conclusive to the like effect. A large amount of additional thermometric evidence has been collected of late years; and this has been most ably digested by the eminent German geographer, Dr. Petermann, who has recently put forward a series of maps for different periods of the year, in which these observations are embodied, and their results made obvious to the eye by the course of the "lines of equal temperature," which in the summer pass between Iceland and the Shetland Islands, a little to the east of north towards Spitzbergen, and thence with more of an easterly bend even beyond the seventy-fifth degree of north latitude. The existence of a warm stream in this direction has been confirmed still more recently by two adventurous officers—Lieutenant Julius Payer, of the Austrian army, and Lieutenant Weyprecht, of the German army—who followed its path last summer in a small sailing vessel hired by themselves, and state that they found open water from east longitude 42° to east longitude 60°, even beyond the seventy-eighth parallel of north latitude, the highest point they reached being north latitude 79°, in east longitude 43°. A Russian expedition under Prince Alexis Alexandrovitch, of which the distinguished *savant*, Von Mildendorf, had the scientific charge, was about the same time exploring the Polar Sea between Nova Zembla and Iceland; and Von Mildendorf has stated to the Imperial Academy of St. Petersburg that "the corvette *Wajag* has proved the extension of the Gulf Stream to the west coast of Nova Zembla, and that we find it on the meridian of Banin Noss (in east longitude 43½°) still of a width equal to two degrees of latitude, and of a temperature of fifty-four degrees Fahrenheit, cooling down only four or six degrees at depths of thirty and fifty fathoms."'

As if to remove all question as to his real opinion the reviewer immediately adds that he fully accepts, not only the great body of facts so 'industriously correlated by Dr. Petermann, but the inference Dr. Petermann draws from them that an attempt to penetrate the polar ice-wall to the north-east of Spitzbergen is more likely to be successful than the search for a passage in any other direction.'

So that (1) Dr. Petermann, regarded by our reviewer as an eminent geographer; (2) Von Mildendorf, whom he regards as a distinguished *savant*; and (3) the reviewer himself, who no doubt does not regard himself as either shallow or stupid, seem all agreed as to the very points which the reviewer has spoken of as involving stupidities and shallow nonsense. Certainly they all agree as to the only points which seem in the least worthy of discussion.

What, then, the reader will ask, is the matter in dispute? Over what momentous question have the angry words quoted above been bandied?

After diligent search for the apple of discord, the student of the review will be led to the conclusion that it is neither more nor less than the *name* 'Gulf Stream.' We have seen that Von Mildendorf calls the warm current which passes by Nova Zembla the Gulf Stream. In this, it appears, he has shown shallowness and stupidity. Dr. Petermann has equally committed himself, or rather has committed a more serious offence. For Von Mildendorf might have used the offensive epithet only through inadvertence; but Dr. Petermann not only uses it, but has the hardihood (we might almost say the cruelty) to maintain that 'it is a matter of no consequence.' Moreover, as our reviewer sadly admits, 'other physical geographers' agree with Dr. Petermann.

The reviewer is so grieved by the defection of the 'distinguished *savant*,' the 'eminent geographer,' and 'the other physical geographers,' that for a moment his confidence deserts him, and instead of applying afresh to them, directly, the lash which has indirectly reached them, he proceeds thus mildly: 'In our *belief*, of which we shall presently explain the grounds, the *real* Gulf Stream has no more to do with the inflow into the polar area than with the ripening of oranges at Naples, or the maintenance of Catholicism at Rome, so that, even if its current were to be entirely diverted by the cutting of a wide channel through the Isthmus of Panama, not only would the climate of the British Islands suffer very little, but a north-easterly stream of warm water . . . . would still mollify the severity of polar cold, and help to render Spitzbergen and Nova Zembla accessible to arctic voyagers.' This belief, in which I cordially concur, would seem to afford excellent reason for rejecting the name Gulf Stream whenever the course of the stream shall thus have been diverted, but scarcely seems to justify the disuse of the name under the actual circumstances; still less would it appear to afford good grounds for using such hard words as 'shallow nonsense' and 'stupidity.' If the course of the Danube were intercepted in Baden, it is tolerably certain that a mighty river would continue to flow past Vienna, Belgrade, and Ismail to the Black Sea; nor would the noble river which flows northward through Germany be much reduced though the Rhine were diverted in the Grisons: yet geographers are satisfied to call these rivers the Danube and the Rhine, not adopting new names at every stage where some new influx changes the size and character of either. And the title 'Gulf Stream' has, in like manner, advantages in point of convenience, which are likely to prevent geographers from rejecting it yet awhile. It may mislead some few into supposing that the whole of the great northeasterly current has passed through the Gulf of Mexico, just as we can conceive that some few students of geography might imagine all the water which flows past Cologne or Coblentz to have come from the Grisons, or all that flows past Nikopolis to have come from Baden. Almost every convenient name, however, is open to some such disadvantage; and the student of oceanic circulation who finds he has been to some degree misled by a name must not mistake the detection of his error for a great geographical discovery.

Majora canamus.

We have hitherto considered surface-currents only. We have not, indeed, considered all the surface currents which traverse the North Atlantic; but the principal streams have been indicated. We must now direct our attention to submarine currents.

It is impossible to consider carefully the nature and distribution of the surface circulation without recognising the fact that there must be currents beneath the surface. It is true that one can conceive the existence of a complete system of oceanic circulation without any movement in the depths of the sea; but when we examine the actual surface currents we find that either the commencement or the prolongation of some currents must necessarily be submarine. For instance, the quantity of water carried by the great north-easterly drift into the Arctic Ocean is very much greater than that which flows out of the Arctic Ocean, by the so-called Arctic current, past Greenland. Examining, indeed, the ordinary current charts, always drawn on Mercator's projection (seemingly because this projection is the very worst that could be devised for the purpose), we might suppose that this Arctic stream was much more extensive than it really is. But what can be expected of a projection which makes Greenland (whose real area is not much greater than that of the Scandinavian peninsula) actually as large as South America. The Arctic current, however, affords yet better evidence of the occurrence of submarine streams, for the extension which passes between the Gulf Stream and the United States, is in places completely lost sight of (the Gulf Stream touching the American shores), and reappears farther on. It is clear that it must have passed under the Gulf Stream in such cases.

Now, the study of the submarine currents has of late years thrown considerable light on the whole question of oceanic circulation, and has supplied the solution of some problems which had formerly appeared altogether perplexing.

We owe to Drs. Carpenter and Wyville Thomson some of the most important facts recently ascertained. Others, however, have shared in the work. I would, indeed, particularly invite attention to the fact that I do not here pretend to give anything like a complete history of recent investigations into the subject. I select only those facts which bear most significantly on the wider relations—the more marked features—of oceanic circulation.

In the first place, a result which had long perplexed physical geographers has been shown to be erroneous. It had been supposed that the temperature of sea-water below a certain depth is in all latitudes constant, and about seven degrees above the temperature at which fresh water freezes. Sir John Herschel, in his 'Physical Geography,' adopted this supposed discovery as well established. Now, let one consequence of such a relation be carefully noted. The surface water in the tropics is warmer than this supposed constant bottom-temperature; the surface water in arctic regions is cooler; at some intermediate latitude the surface water has the same temperature as the water at the bottom. Hence in this intermediate latitude the water is uniformly warm (according to the supposed relation) from the surface to the bottom. We may therefore regard the water in this latitude as constituting, in effect, a constant barrier between the tropical waters and the arctic waters. Without regarding it as absolutely immovable we should yet be compelled to regard it as so far steadfast as to negative the theory of the existence of submarine currents of an importance corresponding to that of the surface currents. Accordingly, the theory put forward by Humboldt and Pouillet to the effect that there is an interchange of waters between polar and equatorial regions was discredited by this supposed discovery.

Drs. Carpenter and Wyville Thomson, however, have been able to show that no such relation exists. There are vast submarine regions of the Atlantic where the temperature of the water is far lower than the constant and uniform temperature believed in by Sir John Herschel. The temperature is, indeed, in places, as low, or very nearly so, as the freezing-point of fresh water, under a surface-temperature 20 degrees or so higher. But in other regions having the same surface-temperature the depths are 10, 12, or 14 degrees higher than that of freezing fresh water. Moreover these relations are constant, so far as the deep water is concerned.

Now, there can be only one interpretation of the circumstances here mentioned. If the depths of the ocean were unmoved by any process of submarine circulation there can be no question that a general uniformity of deep sea temperature would prevail in given latitudes. We should not find the bottom water in one region 13 or 14 degrees warmer than the water in a closely adjacent region. We have only to inquire what is the case in inland seas where no great influx of water of alien temperature can take place, to see that this must be so. Take, for instance, the Mediterranean. Here we learn from Dr. Carpenter's recent researches that 'the temperature at every depth beneath 100 fathoms is found to be uniform, even down to a bottom of 1,900 fathoms; as had, indeed, been previously ascertained by Captain Spratt, although his observations, made with thermometers not protected against pressure, set this uniform temperature too high. In the western basin of the

Mediterranean, as shown by the *Porcupine* observations of 1870, the uniform temperature is 54 or 55 degrees; being, in fact, the winter temperature of the entire contents of the basin, from the surface downwards; and being also, it would appear, the mean temperature of the crust of the earth in that region.' We learn, then, two things—viz., first, that where extensive submarine motions are impossible, a constant submarine temperature may be expected to prevail in the same latitudes; and, secondly, that in the latitude of the Mediterranean the submarine temperature is about 54 or 55 degrees Fahr. Thus, it is clear, in the first place, that the varieties of temperature observed in the depths of the Atlantic must be due to the continual arrival of water of the observed temperatures, at a rate great enough to prevent the deep water from acquiring a constant temperature; and in the second place it becomes possible to tell whence the submarine currents are flowing. If they are cooler than they should be supposing latitude alone in question, then they are travelling from arctic towards tropical regions, and *vice versâ*. On this last point no doubt remains. In a latitude corresponding to that of the Mediterranean basin, the depths of the Atlantic are far colder, even in their warmest portions, than they would be if latitude alone were in question. 'In regard to surface-temperature,' says Dr. Carpenter, 'there is no indication of any essential difference between the Mediterranean and the Eastern Atlantic' in the same latitudes; 'and the thickness of the stratum that undergoes superheating during the summer is about the same. . . . At the depth of a hundred fathoms, in the Atlantic as in the Mediterranean, the effect of the superheating seems extinct, the thermometer standing at about 53 degrees; and beneath this' (in the Atlantic only), 'there is a slow and tolerably uniform reduction at the rate of about two-thirds of a degree for every fathom, down to 700, at which depth the thermometer registers 49 degrees. But the rate of reduction then suddenly changes in the most marked manner; the thermometer showing a fall of no less than nine degrees in the next 200 fathoms, so that at 900 fathoms it stands at 40 degrees. Beneath this depth the reduction again becomes very gradual; the temperatures shown at 1,500, 2,000, and 2,435 fathoms (the last being the deepest reliable temperature sounding yet obtained) being, respectively, 38, 37, and 36½ degrees.'

Thus, there can be no possible question that the depths of the Atlantic are occupied by a vast current much colder than the deep sea temperature due to the latitude, and, therefore, necessarily flowing from the arctic towards the tropical seas.

Such are the broad facts of the Atlantic circulation. We have a surface circulation whose general features are such as have been described, and are generally admitted, though a dispute has arisen as to a question of nomenclature; and then we have a general submarine 'set' of water from the arctic regions towards the tropics, the existence of which is also generally admitted.

But now we again approach a subject of controversy, and one which is certainly better worthy of discussion than that which we considered above. It relates, in fact, to the question how this wonderful system of oceanic circulation is brought about.

Passing over several crude theories which have long since been disposed of, we come first to the theory that the system of oceanic circulation is due to the action of the trade-winds. This theory has been maintained by Franklin and others in former times, by Sir John Herschel in our own, and is warmly advocated in the present day by many whose opinions are not to be rashly contradicted.

Against this theory it has been urged by Captain Maury—'with singular wrongheadedness' according to the Edinburgh Reviewer, but as it seems to me with no small degree of reason—that the trade-winds are neither powerful enough nor persistent enough to account for the great equatorial currents, or therefore for the Gulf Stream. Maury says, 'with the view of ascertaining the average number of days during the year that the north-east trade-winds of the Atlantic operate upon the water between the equator and 25 degrees north latitude, log-books containing no less than 380,284 observations on the force and direction of the wind in that ocean were examined. The data thus afforded were carefully compared and discussed. The results show that within these latitudes—and on the average—the wind from the north-east is in excess of the wind from the south-west only 111 days out of the

365. Now, can the north-east trades, by blowing for less than one-third of the time cause the Gulf Stream to run all the time, and without varying its velocity either to their force or prevalence?' Our reviewer not only dwells on the wrongheadedness of this argument—wholly irresistible as it appears—but asserts that 'the trade-wind origin of the Gulf Stream is about as certain as the rotundity of the earth.' It could have been wished that in place of abusing Captain Maury for wrongheadedness, the reviewer would have devoted a few lines to the demolition of Maury's argument.

Maury himself advanced the relative lightness of the equatorial water as the true reason of the oceanic circulation. But granting that the expansion of the equatorial water under the sun's heat, as well as the resulting buoyancy, would cause an overflow of equatorial water polewards, this overflow would be an exceedingly slow movement, and it would result in an eastwardly instead of a westwardly flow, for the very same reason that the counter trade-winds travelling polewards assume an eastwardly direction.

In the *Student* for July 1868, I advanced another explanation. I urged that the sun's action on the equatorial and tropical regions of the Atlantic, raising immense quantities of water by evaporation, causes an influx of water from below. 'There can be no question,' I then wrote, 'that under-currents arriving in this manner, whether from the north or from the south' (that is from arctic or from antarctic regions), 'would acquire a strong westerly motion (just as the trade-winds do). Thus they would generate *from below* the great equatorial westerly current. In this upflow of cool currents having a strong westerly motion, we find the mainspring of the series of motions. The water thus pouring in towards the equator is withdrawn from beneath the temperate and arctic zones, so that room is continually being made for that north-easterly surface-stream which is the necessary consequence of the continual flow of the great westerly equatorial current against the barrier formed by the American continent . . . . Captain Maury's views seem only to require the mainspring or starting-force towards the west which has been here suggested, to supply a complete, efficient, and natural explanation of the whole series of phenomena presented by the great ocean currents.'

Four or five months later, and while the results on which Dr. Carpenter subsequently based his theory of the oceanic circulation were as yet unpublished, I drew attention in the columns of the *Daily News* to the comparatively limited extent of the influences due to polar cold. This cause, I pointed out, 'scarcely has any influence in latitudes lower than the parallel of 50 degrees.'

In the year 1869 Dr. Carpenter was first led to advocate the theory that the continual descent of cold water in the Arctic Seas is the mainspring of the system of oceanic circulation. He reasoned that the Arctic Seas being exposed to great cold, the surface water 'descends in virtue of its reduction in temperature and increase of density, its place being taken, not by the rising up of water from beneath, but by an inflow of water from the neighbouring area; and since sea-water becomes continually heavier in proportion to its reduction of temperature, this cooling action will go on without the check which is interposed in the case of fresh water.'[1] Thus the water becoming denser and heavier will descend, and 'there will be a continual tendency to the flowing off of its deepest portion into the warmer area by which the polar basin is surrounded; producing a reduction in the level of the polar area, which must create a fresh indraught of surface-water from the warmer area around to supply its place. This, in its turn, being subjected to the same cooling action, will descend and flow off at the bottom, producing a fresh reduction of level and a new indraught at the surface.'

Dr. Carpenter illustrated this theory, or rather the combined action of polar cold and equatorial heat, by an experiment, the plan of which had occurred also to myself, and been described by me in conversation somewhat earlier. 'A long narrow trough having glass sides was filled with water, and a piece of ice was wedged in at one end between its side plates just beneath the top, whilst the surface of the water at the other end was warmed by a piece of metal, of which a part projected from the trough, and was heated by a spirit lamp placed beneath it: thus representing the relative thermal conditions of the polar and equatorial basins. A colouring liquid viscid enough to hold together in the water, while mixing with it sufficiently to move as it moves, being then introduced, the liquid as it impinged on the ice was seen to sink rapidly to the bottom, and then to flow slowly along the floor of the trough towards the opposite extremity, then gradually to rise beneath the heated plate, and then to flow slowly along the surface towards the glacial end, repeating the same movement until the ice had melted.'

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[1] Fresh water expands with reduction of temperature, near the freezing point, and hence, becoming lighter, the descending motion above described is interfered with in the case of fresh water.

It will be observed that in this experiment the effect of cold is not exerted alone, so that it by no means proves that the arctic cold is the chief agent in producing the system of oceanic circulation. Moreover, the conditions of the polar and equatorial basins are in one respect not accurately (or even nearly) reproduced, for the real arctic area is very much smaller, compared with the real equatorial area, than in the case of the experiment. Indeed it appears to me that Dr. Carpenter paid far too little attention to the relative smallness of the arctic area. This may have been partly due to the erroneous ideas suggested by the ordinary maps on Mercator's Projection, in which, as I have already mentioned, the arctic regions are enormously exaggerated. It is almost impossible to study such a map as that which illustrates this paper (see page 210) without feeling that the theory presented by Dr. Carpenter will scarcely hold water, or rather—if this way of presenting the argument be permitted—that the arctic area does not hold water enough to produce the effects described by Dr. Carpenter. For in that map the whole area of the Arctic Ocean is presented;[1] and from out of that area, be it noted, must come the northern supply of descending water, not only for the Atlantic equatorial current, but for the much larger equatorial current of the Pacific, if Dr. Carpenter's theory be sound.

The following letter, written by Sir John Herschel only a few weeks before his lamented decease, has been very widely quoted in favour of Dr. Carpenter's theory; yet if carefully studied it will be found rather to set forth the strength of the theory advocated a year earlier by the present writer. In this letter, at least, Sir John Herschel appears to be disposed, *in so far as he concedes the efficiency of heat, cold, and evaporation*, to incline to the equatorial action as the most important. Answering Dr. Carpenter, who had addressed a letter to him on the subject, he says: 'After well considering all you say, as well as the common-sense of the matter, and the experience of our hot-water circulation pipes in our green-houses, &c., there is no refusing to admit that an oceanic circulation of some sort must arise from mere heat, cold, and evaporation, as *veræ causæ*; and you have brought forward with singular emphasis[2] the more powerful action of the polar cold, or rather, the more *intense* action, as its maximum effect is limited to a much smaller area than that of the maximum of equatorial heat. The action of the trade and counter-trade winds, in like manner, cannot be ignored; and henceforward the question of ocean currents will have to be considered under a twofold point of view.'

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[1] The bounding lines drawn from the pole on the right and left of the white space represent one and the same meridian.

[2] In Sir John Herschel's letters one can often recognise slight touches we will not say of sarcasm (for he was incapable of saying aught that could be considered bitter or unpleasant), but of what may be described as a humorous suggestiveness.

It appears to me that not only is the equatorial or rather tropical action much wider in range, but it is also more intense than the polar action. For, let us consider what happens during the heat of the day over the tropical Atlantic. Here, over an area enormously exceeding the whole arctic basin (we are considering, be it understood, only the northern part of the system of circulation) a process of evaporation is taking place at so rapid a rate as to furnish almost the whole of that rain-supply whence the rivers of Europe and North America (east of the Rocky Mountains) take their origin. There is thus produced a continual defect of pressure, not merely along an equatorial strip, but so far as 20 or even 30 degrees of north latitude, while the downfall of rain which, taking one part with another of the temperate and sub-arctic Atlantic, may be regarded as incessant, continually adds to the pressure in these last-mentioned regions. That on the whole there must result a most effective excess of pressure over the temperate zone of the Atlantic, as compared with the tropical and equatorial portion, seems to me indisputable. Now, if we compare this with the effects of refrigeration over the relatively insignificant arctic area, which as I have said has to supply the North Pacific submarine circulation (if Dr. Carpenter's theory be true), as well as that of the North Atlantic, we can scarcely doubt, as it seems to me, which cause is the more effective. I would venture to predict that if Dr. Carpenter's experiment were tried first with the ice alone to produce circulation, and secondly with the heat alone, the superior efficiency of the latter cause would be at once recognised; but I much more confidently predict that if, as in the experiment I myself proposed, the relative areas of the equatorial and arctic basins were represented, there would be found to be scarcely any comparison between the effects of arctic cold and equatorial heat, so largely would the latter predominate.

It is necessary to mention, however, that the principle itself of the experiment has been objected to, on the ground that the gradation of temperature must always be much more rapid in such an experiment than in the actual case of the Atlantic Ocean. This objection, however, is, in reality, based on a misapprehension. It is sufficient that the difference of temperature at the two ends of the trough should correspond to the difference between the temperature of the arctic and equatorial seas; and it is a matter of no importance whatever that the real rate of gradation should be represented. The case may be compared to the illustration of the descent of water to form springs or the like. Here an experiment would be valid in which the outflow of the illustrative spring was obtained by causing the vent to be so much below the level of the reservoir, though the slope from the reservoir to the vent were very much greater than in the case of any natural spring. Just as in the case of a spring it is *the difference of level*, and not the *rate of slope*, which is effective in causing the rate of outflow, so in the case of the oceanic vertical circulation, it is the actual difference of temperature, and not the rate of gradation, which produces the activity of the circulation.

Another objection has been urged against the 'heat and cold theory' by a very skilful mathematician, Mr. Croll. He reasons on this wise: Since the water which is carried from the equator to the latitude of England[1] (say) must have partaken, when at the equator, of the earth's rotation there, which has a velocity of more than 1,000 miles per hour eastwards, whereas, when it reaches our latitudes, it partakes of a rotation movement reduced to about 620 miles per hour, it follows that, neglecting the drift motions as relatively quite insignificant, friction has deprived the water which has thus travelled from the equator to our latitudes of a velocity amounting to no less than 380 miles per hour. If friction is thus effective, how utterly inconceivable is it, says Mr. Croll, that the descending currents of Dr. Carpenter's theory (or the ascending currents of the evaporation theory) should maintain their motion. Hence, Mr. Croll is an earnest advocate of the trade-wind theory.

The worst of this reasoning is that it proves too much. If friction is so effective, then when the trade-winds flag, as we have seen that they do, the ocean currents ought to be brought to a standstill. Moreover, the submarine currents exist, and the wind theory leaves them unexplained. The fact really is that Mr. Croll's reasoning has no application to a system of fluid circulation, where the advance of one part of the fluid is always made room for, so to speak, by the removal of that which it replaces. We might equally well apply Mr. Croll's reasoning to prove that a river cannot flow because of the friction along its banks, as to show that ocean currents cannot flow within their liquid banks. Indeed, many of the points in dispute in this matter of oceanic circulation may be excellently illustrated by considering the case of a river. I propose to draw this paper to a conclusion by setting forth such an illustration. My readers will not fail to recognise the opinions here severally parodied, so to speak, and so to infer the theory which I regard as affording, on the whole, the best explanation of the observed relations.

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[1] I present the general nature of Mr. Croll's reasoning, without following him in details.

'Shallow persons,' might one say, 'have launched all sorts of stupidities upon the Mississippi River. Physical geographers have deluged the world with their assumptions respecting it; theorists of all kinds have floated their notions upon it. One says that it brings down, past Baton Rouge and New Orleans, the drainage of half the United States; others ascribe to it the *detritus* around the delta of that great river which flows into the Gulf of Mexico; yet others consider that it breeds the fogs infesting the path of the great stream which flows from Vicksburg to Placquemines.' All this is utter nonsense. The Mississippi has no more to do with the great stream flowing through Louisiana than with the Thames at London. The real Mississippi is a stream of singular purity, and presents other characteristics clearly recognisable as far as its junction with the Missouri; but in the stream which runs past St. Louis none of the characteristics of the Mississippi can be traced. Here, to all intents and purposes, the Mississippi comes to an end. As for the cause of the motion of the great stream itself, there can be little question. Some have urged that it is due to the gradual slope of the land; but in all the experimental illustrations of the effects of such slope which we have yet seen, the inclination has been monstrously exaggerated. If slope were the cause of the river's flow, then unquestionably the effective part of the action must reside in the Rocky Mountains, and not in the great reaches of the river. We admit that the chief bulk of the river lies in the great reaches; but this fact has no bearing, we assert, on the question at issue. However, it is demonstrable that no cause of this sort can be in question. For *let the following reasoning be carefully marked.* In Wisconsin, in 40° north latitude, the river partakes of the earth's rotation motion, there equal in rate to about 800 miles per hour; in Louisiana, in 30° north latitude, the river still partakes of the earth's rotation movement, here equal to about 900 miles per hour. Hence, were it not for the friction exerted by the banks, the water of the river in Louisiana would be flowing at the rate of 100 miles per hour westwards. If, then, friction deprives the river of this enormous velocity—as it obviously does— how much more must it deprive the river of the minute velocity of four or five miles per hour due to slope or inclination. It is certain, therefore, that the flow of the stream is due to the prevalent northerly winds of the so-called Mississippi valley. There are not wanting those, indeed, who assert that this cannot be the case, because northerly winds are *not* prevalent in this region. But the singular wrong-headedness of this reasoning renders reply unnecessary. That the flow of the great stream is caused by these winds is as certain as the rotundity of the earth.

From *English Mechanic* for July and August 1872.

ADDENDUM.[1]

It is impossible but that on a subject so difficult and complicated as that of oceanic circulation, different views should be entertained by students of science. And it is clear that in the present stage of the inquiry no useful purpose could be fulfilled by making the problem a matter for controversy. Dr. Carpenter himself has shown that much more is to be gained by observation than by reasoning on imperfect knowledge. If I venture to remark that his deep-sea researches have led to the most important contribution which has been added for many years to our information respecting oceanic circulation, he will not, I trust, consider that I am passing beyond the bounds of controversial courtesy. But I am, indeed, not anxious to treat the matter as one for controversy in any sense. It will be perceived by those who have read my remarks on the subject, that I have rather put them forward as suggestions than as indicating theories which can be maintained with any degree of assurance, far less with conviction. Nor does it seem to me likely that one explanation can suffice to account for all the phenomena recognised in oceanic circulation. This is a case, if ever such case were, in which more causes are in operation than one; so that it may very well happen that excellent arguments can be adduced in maintenance of different views. If, therefore, I enter on the defence of what I have already written on the subject, it is not with the wish to show that one particular explanation of oceanic circulation is correct, and all others erroneous. If I am desirous of dealing with the considerations urged by Dr. Carpenter, it is not because they seem to him to militate against the views I have to some extent advocated. What I wish to show is that I have not addressed your readers on the subject of oceanic circulation without making myself familiar with the facts which bear upon that subject, and at the very least, with those comparatively fundamental facts to which attention has been invited.

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[1] This paper was written in reply to comments by Dr. Carpenter on the former paper. The nature of these comments will be inferred from my reply; in fact I quote the most important passages.

And here I would remark that one who writes so much and so often as I have had occasion to do on this and kindred subjects, is placed to some degree at a disadvantage. He cannot, on the one hand, assume that the readers of any particular essay have also read all that he has written on the subject; yet, on the other, he cannot assume that none have done so, and that he is therefore free to repeat (in a more or less modified form) much that he has formerly urged. I was, perhaps, somewhat too careful in writing for your pages to avoid touching at any length on any parts of the subject which I had more particularly dealt with elsewhere; and accordingly I have laid myself open to a method of attack, which in reality involves the suggestion that I have written without due consideration even of the elements of my subject. I have no doubt that Dr. Carpenter has no wish to imply this directly, yet indirectly it is implied in every paragraph of his reply. I shall be able to show, however, that every one of the points touched on by Dr. Carpenter had been fully considered by me—and, for the most part, several months before he had turned his attention to this subject.

First, there is the remark that I have left out of view the circumstance that if there is excess of evaporation in the intertropical area, the excess ought to show itself, as in the Mediterranean, in an *increase* of specific gravity, whereas the specific gravity of the equatorial water is *lower* than that of tropical water. Now, it is unquestionably true that the effect of evaporation is to increase the specific gravity of sea-water; but it is equally true that the effect of the heat which causes the evaporation is to diminish the specific gravity. The point is considered in my essay entitled 'Is the Gulf Stream a Myth?' in the first series of

'Light Science for Leisure Hours.' 'We recognise,' I there say, 'two contrary effects as the immediate results of the sun's action. In the first place, by warming the equatorial waters it tends to make them lighter; in the second place, by causing evaporation it renders them salter, and so tends to make them heavier.' And I proceed to inquire which cause is likely to be the more effective, arriving at the conclusion that the water is made lighter. The case, indeed, appears to me to be altogether different from that of the Mediterranean Sea cited by Dr. Carpenter. In the Mediterranean we have the same heating action as on the Atlantic in the same latitudes, but not the same relatively enormous quantity of water freely communicating with the region so heated. We have, then, in the Mediterranean evaporation as everywhere else, and evaporation to the same degree, appreciably, as elsewhere in similar latitudes; but evaporation *not* compensated as in the open Atlantic by the effects of free communication with surrounding water. Hence we have in the Mediterranean an increase of saltness; in other words, an increase of specific gravity. And precisely because this increase takes place in the Mediterranean, whereas the water of the Atlantic in the same latitudes, exposed to the same average degree of heat, is not rendered heavier, it may be maintained not unreasonably that the water of the equatorial Atlantic being unconfined, will in like manner not be rendered heavier by evaporation. It seems to me that we have here a positive argument of great weight in *favour* of my views.

But independently of this I would ask whether it can be questioned that enormous evaporation *does* take place over the equatorial area. This is what I contend for, and I should have imagined that few would undertake to deny the proposition.

In passing, I must remark that I do not adopt the distinction between equatorial and tropical water which Dr. Carpenter appears to recognise. I have in view the evaporation over an enormously larger area than he considers—no less an area, in fact, than the whole ocean between latitudes 40° north and south of the equator (at the equinoxes, and varying according to the season). It by no means follows that because the equatorial current does not cover this enormous area, therefore the relation which I have suggested as the mainspring of oceanic circulation has not that extent. On the contrary, while it is on the one hand certain that there is an excess of heat over this enormous area, it is on the other almost a necessity of my theory that the resulting current should be found running along the middle only of the great region of evaporation.

This brings me to Dr. Carpenter's second objection, that if the removal of equatorial water draws in polar water from the *bottom*, the whole intermediate stratum should first rise towards the surface. I do not hold the view thus demolished, but simply that the inflow is from below. The question whether the inflow would be from above or below was dealt with by me in a paper on 'Oceanic Circulation' in the *Student* for

July 1868. I do not urge this as a proof that Dr. Carpenter's objection is invalid. My reasoning may admit of being refuted. But I wish to show that the objection is not a new one to me. The inflow may be from below without being from the bottom. If it were from the bottom it would not have the effects I have ascribed to it, that is, it would not result in a westwardly-flowing current. What I conceive is that since the whole tropical and equatorial area is a region of excessive evaporation (as surely no physicist will deny), there is over the whole region a depression of the ocean level. This depression may be, or rather must be, exceedingly minute; but the total quantity of water thus, as it were, wanting, must be enormous. The difference must, by the laws of fluid equilibrium, be supplied, and though the immediate supply in equatorial regions may come from tropical regions, the actual source of the total supply must be sought for in higher latitudes. That the water drawn in under these circumstances would traverse the surface of the Atlantic, is by no means proved by the fact that the eminent mathematicians cited by Dr. Carpenter consider that an in-draught to replace water 'swept off from the surface' by trade-wind action would be a surface current. The two cases are wholly dissimilar. I must, however, admit that *my* case is one of extreme difficulty regarded as a problem in hydrodynamics. It is so difficult that I do not believe it can be solved even after the very imperfect fashion in which hydrodynamical problems have hitherto perforce been dealt with.

When the physics of hydrodynamics have been treated by mathematicians like the physics of astronomy, or rather when they *can* be so treated, it may be possible to deal with this problem. Unless I greatly mistake, however, in such a *then* we shall find a *never*.

I do not see how the action of the cause I have considered is affected by the circumstance that the equatorial heat does not show any effects below 200 fathoms; for the cause is in its very nature a surface one. But I would remark that, so far as continuity of action is concerned, the equatorial heat seems at least on a par with the polar cold. For as the aqueous vapour rises it finds its way to regions where the atmospheric circulation is at work to carry it away (it is only the surplus quantity which is condensed into clouds, and even these are in great part carried away); and thus the process of evaporation can hardly be exhausted. Even at night, though in a modified manner, the evaporation must continue. But the action of the polar cold, though it is continuous in the sense that the increase of cold extends to great depths, yet has this great difficulty to contend with, that the descending water must perforce wait until room is made for it by the slow removal, the *creeping away*, as it were, of that which it replaces. That this cause, *per se*, can ever become one of sufficient activity[1] to generate a complete system of vertical oceanic circulation seems at the least open to grave question. It appears to me also that when applied to the North Pacific this theory fails. Very little water can pass through Behring's Straits, and beyond Behring's Straits there is an island-locked and shallow sea of enormous area, altogether unlike the deep North Atlantic.

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[1] In passing I may notice that I did not suppose Sir J. Herschel to be humorous in reference to the intensity of the polar action, but in his use of the word 'emphasis.' I should not have touched on the point, did I not thoroughly sympathise with the emphatic utterance of speculative or theoretical opinions.

I would further point out that the interesting fact above mentioned, namely that the equatorial heat exerts no perceptible effect at a depth exceeding 200 fathoms, is in reality almost a necessity for my theory. For if the whole of the equatorial ocean were heated, and, therefore, of reduced specific gravity, the water arriving from higher latitudes would flow to the bottom, and so have to force up the intervening strata, in order to produce the observed effects; and this may be regarded as impossible. As it is, such colder and heavier water would be in dynamical equilibrium within a very short distance of the surface.

Next, as to the question of rainfall. Dr. Carpenter considers that I have overlooked the considerations (1) that the rainfall of Europe and North America may be accounted for by the evaporation in the Mid-Atlantic, beyond the region of the trade-winds, say between 20° and 40° north latitude; and (2) that there is an enormous rainfall in the region of equatorial calms, which Sir John Herschel attributes to the deposit of waters taken up by the N.E. and S.E. trades. To this I must reply that in my essay on Rain, in the *Intellectual Observer* for December 1867, I have weighed the whole question of rainfall at least with great care, and with constant reference to the best sources of information. One circumstance I there note which seems at a first view (or rather viewed as Dr. Carpenter appears to consider the matter) much more fatal as an objection to my theory than either of those noted by Dr. Carpenter; viz., that according to the observations of Humboldt and others, the annual rainfall is at a maximum at the equator, and diminishes with increase of latitude. But the whole question is, where does all this rain come from? If it comes from tropical and equatorial evaporation it will surely not be argued that what falls in or near the place of evaporation itself represents the total amount of such evaporation. It is unquestionable, I conceive, that the rainfall is only the excess of the aqueous vapour poured so copiously into the air from the whole of this region. It is the quantity which the air, as it were, rejects. It is a matter of little importance where the rainfall of higher latitudes comes from, though it should be noticed that the views of Dové, Kaemtz, and other leading meteorologists respecting the winds and rains of high and low latitudes, support my remark about the great rivers.

Now we have in the phenomena of the zone of calms a crucial test of Sir J. Herschel's theory as to the origin of the equatorial rains. It appears to me that this test altogether negatives Herschel's theory. If the moisture to which these equatorial rains are due came from the trade-wind regions, we should certainly not expect the fall of these rains to be associated in any marked degree with the progress of the equatorial day; or, if at all, then the cooler parts of the day, when the point of saturation is lower, would be the time of precipitation. With the mid-day heat would come a cessation of precipitation. As a matter of fact the contrary is the case. The sun (we are told by Dové, Kaemtz, Humboldt, Maury, Buchan, and many more) rises commonly in a clear sky in equatorial regions. As the day proceeds clouds form, and towards mid-day they grow dense. It is at noon that heavy showers fall, and towards evening the skies again become clear. Now, any one who has noticed what happens on calm summer days in any well-watered region can see that the equatorial phenomena represent the same processes on a greatly enlarged scale. On a summer's day in such regions we see how scattered cumulus clouds begin to form in early morning, become larger and more numerous as the day proceeds, and in the afternoon begin to be transformed into cumulo-stratus. The explanation is simple. The sun's heat has caused aqueous vapour to rise into the air, until there is so much that not very far above the earth's level the saturation point is reached. The further rise of the vapour is followed by the process of condensation into clouds, much heat being given out in the process, causing the air to expand in the neighbourhood of the clouds so formed, and thus giving to these clouds their peculiar rounded tops. (At least this feature seems better explained thus than by De Saussure's theory.) Now suppose the conditions changed to those existing at the equator. The supply of vapour is very much greater, the saturation point is very much higher near the sea-surface, and the contrast between the conditions prevailing there and in the region where condensation begins is very much more marked. The air above the equatorial and tropical seas contains, in the form of invisible aqueous vapour, an enormous quantity of water; this vapour rises and extends itself, its place being continually supplied by fresh evaporation. What must happen when the process has continued for several hours, but precisely what is observed to happen? There is an overflow, so to speak, resembling, only much more marked, that which causes the formation of our summer clouds. Enormous cloud-masses are formed, which cannot be carried away by the atmospheric circulation (very high above the calm zone), so fast as they are formed. Hence follows excessive accumulation, presently resulting in precipitation, accompanied by remarkable electrical phenomena.

But to suppose that the whole quantity of water evaporated at the equator and in tropical regions is precipitated *there* in the form of rain, corresponds to such a supposition as that the water overflowing a dam includes all that has risen to the level of the dam.

I should not be greatly concerned if the result of the experiments I spoke of should not accord with my prediction. But merely to put ice in water capable of melting it, is not in any sense to represent the conditions of the actual case. The addition of water from the ice as it melts is not in accordance with these conditions. It cannot surely be maintained that the oceanic circulation depends on the addition of water from the melting of ice; and yet I apprehend that the melting of ice is no unimportant feature of Dr. Carpenter's experiment. At any rate, the ice does melt, and the movement comes to an end when all the ice has melted away. Let the ice be packed outside the arctic end of the canal, so as merely to produce a refrigeration corresponding to what actually takes place with water carried into arctic latitudes, and I conceive that a very feeble circulation would result. Under the actual circumstances, the melting of the ice produces effects much more nearly corresponding to those due to rainfall than to the mere effects of arctic cold. The very activity of the circulation shows that the water which moves towards the ice does not undergo refrigeration. Water does not cool quite so quickly. It is the melted ice-water which descends; and nothing takes place in the arctic regions which corresponds to this continued addition of water to that already circulating. Otherwise, the arctic ice would be continually diminishing, which, of course, is not the case.

It will be gathered that I agree entirely with the opinion which Sir W. Thomson expressed as to the reason why heat is necessary for Dr. Carpenter's experiment. Heat is necessary, because the ice *must* be melted to make the experiment succeed. But comparing the effects of heat and refrigeration (*not* of heat and the continual inflow of ice-cold water), I conceive that heat would be found altogether the more effective.

Lastly, as to the wind theory of the Gulf Stream, Dr. Carpenter remarks that, so far as he knows, I am 'the only man of science in this country agreeing with Capt. Maury in attributing the Gulf Stream to some other cause than the impelling force of the trade winds.' He must be aware that there are not half a dozen students of science in this country who have expressed definite opinions on the subject after a thorough and independent inquiry into the evidence. Amongst those who maintain the wind theory there is not one, so far as I know, with whom Dr. Carpenter is in agreement. Mr. Laughton disputes the very principle of Dr. Carpenter's reasoning, holding that the change of temperature from equator to poles proceeds too slowly mile for mile to produce the effects which Dr. Carpenter indicates. Mr. Croll, in like manner, has expressed his complete dissent from Dr. Carpenter's reasoning. So also has Mr. Findlay. I believe these gentlemen to be mistaken, and I conceive that I have been able to put my finger on the precise point where their respective lines of reasoning fail. But, if Dr. Carpenter is to take general consent as an argument, and to maintain that I am wrong because he knows of no one who agrees with me, I may as well point out that he is entering into a very questionable alliance, so far as his special views are concerned. So far as I know, all the Continental students of science who share our common views as to vertical circulation, reject the wind theory as *solely* sufficing to account for the Gulf Stream. Again, he sets Sir J. Herschel's opinion (thirty years ago) that 'the Gulf Stream is *entirely* due to the trade winds' as almost conclusive against me. It is, at least, not new to me, since it is cited in every paper I have written on the subject. But is there no evidence to show that Sir J. Herschel abandoned the view he formerly entertained? I would ask what Sir John Herschel implies when, in his letter to Dr. Carpenter, he writes, 'The action of the trade and counter-trade winds, in like manner, cannot be ignored; and henceforward the question of ocean currents will have to be considered under a twofold point of view.' The word 'henceforward' implies very distinctly that Sir J. Herschel was entertaining a new opinion—that is, an opinion new to him; and I think Dr. Carpenter would find it difficult to demonstrate that this new opinion would not have enforced the omission of the word *entirely* from the sentence quoted by Dr. Carpenter.

I need hardly say that I do *not* agree with Captain Maury, whose theory of oceanic circulation appears to me to be wholly untenable. Nor do I for a moment assert that the winds play no part in producing oceanic circulation. I may have been mistaken in attaching so much weight as I have to Maury's evidence as to the trade-wind zones, though it is known that science owes more to him than to any man for our present knowledge of the winds prevalent in certain regions; and when I first wrote on the Gulf Stream there was no evidence on the subject even approaching Maury's (or that collected by Maury) in accuracy and completeness. But there is one argument which those who have adopted the trade-winds as the primary cause of the Gulf Stream appear to me to have overlooked, and it is on this argument that my own view has been chiefly based. The trade-wind zone of the northern hemisphere is not constant in position; but travels northwards and southwards, with the northerly and southerly motion of the sun in declination. The change in the position of the zone of calms is not, indeed, so great as is stated in Buchan's meteorology, where it is said to travel from 25° north to 25° south of the equator; but it is considerably greater than was supposed by Dové, Kaemtz, and others. If we set the extreme shift of the northern trade-zone at ten degrees we are certainly not overrating it. Taking this zone as extending in spring or autumn from 10° to 25° north latitude, we should have it in winter extending from 5° to 20°, and in summer from 15° to 30°, the only part common to these two ranges being that from 15° to 20°—that is to say, the northern five degrees of the winter zone, and the southern five degrees of the summer zone, each zone being 15° wide. Now, if any one will mark these zones on the North Atlantic, he will find that while the zone of winter trades would produce a current flowing into the southern half of the Gulf of Mexico, the zone of summer trades would produce a current flowing into the northern half. The former would produce a current flowing as the Gulf Stream actually flows; the latter would produce a current flowing precisely in the opposite direction. This being the case, I do not find the evidence for the trade winds as the sole or even the main cause of the Gulf Stream altogether convincing. The case does not, for instance, seem quite 'as clear as the rotation of the earth.' It seems, also, not undesirable to mention that the equatorial current and the Gulf Stream are not mere drift-currents, and that on a careful estimation of the frictional action of such winds as the trades on the surface of the ocean, the action will be found quite unequal to the propulsion of so vast a body of water as is actually carried westwards (not, by the way, *before* these winds). Until difficulties such as these have been removed from the trade-wind theory *as solely sufficient* to account for the Gulf Stream, I think I would rather be the only student of science opposing that theory, than one of a phalanx, however large, maintaining it. There is, however, no such phalanx; the subject being regarded by nearly all students of science as a very open one.

English Mechanic, Aug. 30, 1872.

THE CLIMATE OF GREAT BRITAIN.

If there is one feature in the material relations of a country which may be considered as characteristic—as of itself sufficient to define the qualities of the inhabitants, and the position they are fitted to occupy in the world's history—it is climate. 'It includes,' says Humboldt, 'all those modifications of the atmosphere by which our organs are affected—such as temperature, humidity, variations of barometric pressure, its tranquillity or subjection to foreign winds, its purity or admixture with gaseous exhalations, and its ordinary transparency—that clearness of sky so important through its influence, not only on the radiation of heat from the soil, the development of organic tissue and the ripening of fruits, but also *on the outflow of moral sentiments in the different races*.' I do not propose, however, to deal with the constitution of the climate of Great Britain under this general view. To do so, indeed, would require somewhat more space than can in this volume be conveniently allotted to a single subject. I wish chiefly to consider the subject of temperature (mean annual and extreme winter or summer temperature); though I shall have a few words to say respecting that feature of our climate which most foreigners consider to be its chief defect—the want of transparency or clearness in our skies as compared with those of some other European countries.

The mean annual temperature of a country is less important to the welfare of the inhabitants than the extreme range of temperature exhibited in the course of the year. Of two countries which have the same mean annual temperature, one may have a climate most admirably adapted to the welfare of its inhabitants, while the other may have a climate offering such fierce and violent extremes of heat and cold that its inhabitants resemble the unfortunates described by Dante, doomed

> '— a soffrir tormenti e caldi e geli.'

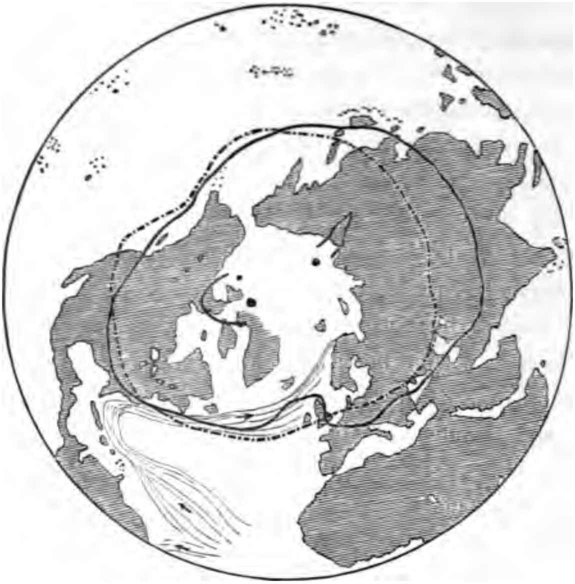
However, I shall deal first with this feature—mean annual temperature—as affording a starting-point from which to proceed to other considerations.

If the surface of the earth were perfectly uniform, or symmetrically distributed into districts of land and water arranged in zones along latitude-parallels, and if the strata of the soil were throughout of like density, radiating power, and elevation, the lines of equal mean temperature would be parallels of latitude. This hypothetical condition of things is, we know, very far from representing the true condition of the earth's surface. Land and water are distributed in a manner which hardly presents the semblance of law; elevations and depressions, not merely of areas of limited extent, but of whole countries, are exhibited in each hemisphere; and endless diversities of soil, contour, and distribution, disturb that mathematical uniformity and exactness which could alone produce the co-ordination of climates under latitude-parallels.

It is to Humboldt that we owe the valuable proposition that maps of the world should exhibit *parallels of heat*, as well as latitude-parallels; and no atlas is now considered complete without maps in which *isotherms*, or lines of equal mean annual temperature, *isochimenals* or lines of equal winter heat, and *isotherals* or lines of equal heat in summer, are exhibited. These lines are usually presented in maps on Mercator's projection, an arrangement which has some advantages, but is not, on the whole, very well suited to exhibit the true conformation of the isothermal lines—the study of which, it has been well remarked, constitutes the basis of all climatology.

In Figs. 1 and 2, the northern hemisphere of the earth is presented on a projection (the equal surface) which has been discussed in my 'Essays on Astronomy.'

FIG. 1.

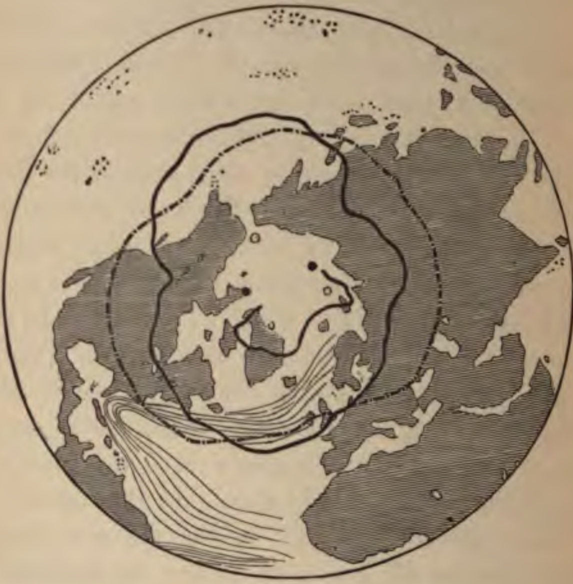


Northern hemisphere on an equal-surface projection, showing curves of mean annual (- - -) and midwinter (—) temperature through London.

The smallness of the scale would not readily permit of the introduction of the system of isothermal lines usually presented, therefore I have only introduced the isotherm which passes through London. In both figures this isotherm is represented by a dotted closed curve passing across the south of England, thence across the Atlantic in a south-westerly direction, and across the continent of America nearly on the latitude of New York. After it has entered the Pacific Ocean, the isotherm passes somewhat northwards, but trends southwards again as it nears the Asiatic continent, reaching its greatest southerly range in the Sea of Japan, traversing Asia nearly on the latitude of the Aral Sea, and thence passing somewhat northwards through the Crimea, Vienna, and Brussels to London. Along its whole extent the isotherm nowhere has a higher latitude than where it crosses the British Isles; in other words, the mean annual temperature of Great Britain is higher than that of any country lying between the same latitude-parallels. The advantage of this arrangement is second only in importance to that which England will be seen to possess when we come to consider the extreme range of temperature during the year. In fact, England is thus brought to the centre of the true temperate zone of the northern hemisphere; and the consideration of Figs. 1 and 2 will show that the isotherm of London approaches as near to the tropic of Cancer in one part of its course as to the Arctic circle in another.

Before leaving this part of the subject, let me note a circumstance, not immediately connected with the climate of Great Britain, but geographically interesting. In examining the polar presentation of the London isotherm, we see that in two parts of its course it exhibits a tendency to travel northwards, and becomes, in fact, *convex* towards the pole. If we laid down isotherms of greater mean temperature—that is, nearer the equator—we should find this peculiarity gradually diminishing. But if we laid down isotherms of lower mean temperature, we should find the convexities

FIG. 2.



Northern hemisphere on an equal-surface projection, showing curves of mean annual (- - -) and midsummer (—) temperature through London.

gradually becoming sharper and more defined, approaching each other more and more nearly, until finally they would meet, and the isothermal curve be divided into two irregular ovals. Proceeding to trace out curves of still lower temperature, we should find the two ovals closing in towards two poles of cold. These are indicated in Figs. 1 and 2 by two black spots, one north of the American, the other north of the Asiatic continent. It is to be noted, however, that at the American pole the mean annual temperature is not quite so low as at the Asiatic pole, the former temperature being 3½°, the latter 1° Fahrenheit.

Returning to our subject, let us consider the all-important question of range of climate. The effects of climate, unimportant to the stronger inhabitants of a country, but largely influencing the health and comfort of the majority, are chiefly felt through the changes that occur during the year. Now, we have seen that the line of mean annual temperature of England departs in a very marked manner from coincidence with a latitude-parallel; but we shall find the lines indicating the extreme temperatures of the year much more irregular; and the peculiarity of climate, which their conformation illustrates, much more important.

In Fig. 1 the *isochimenal*, or the line of equal winter heat, through London, is indicated by a strongly-marked closed curve. Its form is remarkable. It passes nearly in a north and south direction, along the length of England and Scotland, approaches singularly near to Iceland, but turns sharply southwards, and travels across the Atlantic in a direction which brings it to the American continent near Washington. Still approaching the tropics, it travels through the northern parts of Texas, where it reaches its greatest southerly range. Passing gradually northwards to the neighbourhood of the Aleutian Islands, it thence trends southwards again, passes through the Corea, traverses the Asiatic continent nearly on the latitude-parallel of Nankin: thence travelling slightly northwards, it crosses the southern part of the Caspian Sea, the Black Sea, and the north of Turkey, passing through Venice and Paris to London. On the continents the isochimenal falls outside (that is, south of) the annual isotherm, while on the oceans the reverse holds. The projection of the isochimenal thus appears as an irregular oval, whose greatest length lies on the continents.

We see here, again, the indication of a tendency to form two curves, and thus of the presence of two poles of extreme winter cold in the northern hemisphere. The isochimenals of greatest cold hitherto traced in the two continents are shown by two broken curves in Fig. 1. The cold of the Asiatic curve is very much greater than that of the American, the former curve marking a winter cold of −40° Fahrenheit (72° below freezing), the latter a winter cold of −26° 5′ only—if one may apply such an adverb to a cold of 58° 5′ below freezing. Professor Nichol remarks that, 'if a polar projection were made of these regions for January, it would be found that the two coldest spaces of these continents form a continuous band passing across the pole of the earth.' I cannot but think that this is a mistake. I believe that if the isotherms traced, in part, in Fig. 1 could be completed, they would be found to form two ovals. The American oval would enclose the American pole of mean temperature, but very eccentrically, showing that the pole of extreme winter temperature lay westwards and southwards, probably near Victoria Land. The Asiatic oval would not probably enclose the Asiatic pole of mean temperature; and the position indicated for the Asiatic pole of extreme winter cold lies on or near the Arctic circle, where it is crossed by the river Lena. At the true pole of the earth the extreme winter cold is probably not nearly so intense as the cold at either of the points here indicated.

From the direction of the isochimenal through London, it is evident that the Eastern Counties and Kent experience the coldest winters of all places in the British Isles, while Cornwall and the south-westerly parts of Ireland enjoy the mildest winter climates. In fact, winter in Cornwall is not more severe than in Constantinople; and in south-west Ireland the winter is still milder, approaching in this respect to the winter climate of Teheran.

The contrast, when we turn to the *isotheral* of London, is remarkable. Instead of travelling nearly northwards, this curve passes in a south-westerly direction, reaching its greatest southerly range in the central part of the Atlantic Ocean; thence it travels with a northerly sweep through Nova Scotia and Canada, till it reaches its greatest northerly range near the Rocky Mountains.[1] Thence it turns sharply southwards, crosses Vancouver's Island, sweeps nearly to latitude 45° in the central part of the Pacific, whence passing slightly northwards it crosses the southern part of Saghalien Island. Here it turns sharply northwards, crosses that very district of Siberia which, in Fig. 1, is occupied by the isochimenal of intensest winter cold, traverses Siberia, and passes near St. Petersburg, through Berlin and Amsterdam to London.

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[1] It is noteworthy that the minimum distance of the isotheral from the North Pole here attained is exactly equal to the minimum distance of the isochimenal from the equator.

The relations thus presented by the isotheral of London are precisely the reverse of those exhibited by the isochimenal. The isotheral forms a closed irregular oval, whose greatest length lies on the two oceans: here it falls outside the line of mean annual heat, while on the Continent it falls far within this line.

In another respect the isotheral presents a noteworthy contrast to the isochimenal. While the latter encloses an area largely exceeding the area enclosed by the mean annual line, the isotheral encloses an area noticeably smaller.[1]

A tendency to break up into two curves is exhibited in the isotheral, even more markedly than in the two other curves. But singularly enough, here, where one would expect more certain information of the existence of poles of cold, since so much more of the northern hemisphere can be traversed in summer than in winter, we have no satisfactory evidence. In fact, the irregular curve marked in near the pole in Fig. 2 is the most northerly isotheral yet determined. The temperature corresponding to this isotheral is 36° Fahrenheit, or four degrees above freezing. From a consideration of the form-variations of the isotherals as they travel northwards, I have been led to the opinion that there exist *three* poles of summer cold, and that these fall not very far from the positions indicated by the small dark circles in Fig. 2.

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[1] Here an important advantage of the isographic projection is exhibited. The relation pointed out is altogether obliterated in Mercator's projection, and could only be roughly inferred from any but an isographic projection.

From the direction of the isotheral line through London, it is evident that along the south-eastern coast of England the heat of summer is greater than in any other part of the British Isles. On the other hand, the northern parts of Scotland, which we have seen enjoy a winter climate fully as warm as that of London, have a much cooler summer climate. The south-western parts of Ireland exhibit a change even more remarkable. For whereas the winter climate in these parts is the same as that of Persia, the summer climate is the same as that of the very portion of Siberia in which (most probably) the greatest cold ever observable in our northern hemisphere is experienced in winter. The summer of the Orkney Islands, again, is no warmer than that of the southern parts of Iceland.

It appears, then, that the inhabitants of England enjoy three notable advantages as respects climate. First, a higher mean annual temperature than that of any other country so far from the equator; secondly, a moderate degree of cold in winter; and lastly, a moderate degree of heat in summer. The last two advantages resolve themselves into one, viz. small range of temperature throughout the year. Our range of climate is from about 36° in winter to 62½° in summer, or in all, 26½° Fahrenheit. Compare with this the climate of the country near Lake Winnipeg, with a winter cold 4° below zero, and a summer heat scarcely inferior to that of London; so that the range of climate is no less than 65°. Yet more remarkable is the variation of climate in parts of Siberia, near Yakutsk; here the range is from −40° in winter to 62° in summer—a variation of 102°, or four times the variation of our London climate. Other parts of the British Isles have, however, a yet smaller range even than that of London. Thus in the south-western parts of Ireland, and in the Orkney Isles, the variation is less than 19°.

Nor is it difficult to assign sufficient reasons for the mildness of the British climate—for our warm winters and cold summers. It will appear, on examination, that nearly all the constant causes affecting the temperature of a climate operate to raise the mean temperature of our year, while, of variable causes, those which tend to generate increased heat operate in winter, while those which have a contrary effect operate in summer.

Humboldt enumerates among the causes tending to exalt temperature the following non-variables:—The vicinity of a west coast in the northern temperate zone; the configuration of a country cut up by numerous deep bays and far penetrating arms of the sea; the right position of a portion of the dry land, *i.e.* its relation to an ocean free of ice, extending beyond the polar circle or to a continent of considerable extent which lies beyond the same meridional lines under the equator, or at least in part within the tropics; the rarity of swamps which continue covered with ice through the spring, or even into summer; the absence of forests on a dry, sandy soil, and the neighbourhood of an ocean-current of a higher temperature than that of the surrounding sea.

All these causes, it will be observed—except the neighbourhood of a tropical continent on the same meridian—tend to increase the mean heat of the climate in England. The great Gulf Stream probably exercises a more important influence than any of the others. Its position is indicated in Figs. 1 and 2. Humboldt attaches a high importance to the presence of a tropical continent on the same meridian; and he considers that the climate of Europe is warmer than that of Asia, because Africa, with its extensive heat-radiating deserts, lies to the south of Europe, while the Indian Ocean lies to the south of Asia. There are objections, however, to the reasoning he adopts. In the first place, if the heat-radiating power of a continent really influenced the country lying to the north, it should tend to lower rather than raise the temperature, for the ascending currents of air would strengthen the currents of colder air pouring in from the north, and these currents—on Humboldt's assumption that the country directly to the north is that affected—would lower the mean annual temperature. It would only be exceptionally that the warmer returning currents would descend, and thus exalt the temperature. It seems clear, however, that Asia is the continent chiefly affected by the heat-radiating power of Africa; since the cold currents from the north travel eastwards, while the warm return-current has a westerly motion. We should thus attribute the milder climate of Europe rather to the influence of the tropical parts of the Atlantic Ocean, than to the cause assigned by Humboldt, and we should invert the effects he attributes to oceans and continents respectively. With this change—somewhat a bold one, I confess[1]—we may say that all the non-variable causes tending to exalt temperature operate in England's favour.

The constant causes tending to lower temperature are simply the converse of those above considered.

Of variable causes increasing temperature, the principal are a serene sky in summer, and a cloudy sky in winter. It may appear, at first sight, paradoxical to assign opposite effects to a cloudy sky. It must be remembered, however, that clouds, considered with reference to temperature, have two functions: they partially prevent the access of heat to the earth, and they partially prevent the escape of heat from the earth. Now, in summer the first-named influence is more important than the second: the days are longer than the nights; that is, the earth is receiving heat during the greater part of the time in summer. A cause, therefore, which affects the receipt of heat is more important than a cause affecting the escape of heat. On the other hand, in winter the nights are longer than the days, and the influence of clouds in preventing the escape of heat becomes more important than their effect on the receipt of heat.[1] In fact, we may compare the influence of clouds to the effects of certain kinds of clothing; flannel, for instance, is as suitable an article of dress for the cricketer as for the skater.

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[1] Not unsupported, however, by good authority. Thus Professor Nichol, speaking of the climate of Europe, writes: 'The air that rises in Africa blows rather over Asia than Europe. The cradle of our winds is not in Sahara but in America.' Again, Kaemtz notices, that if the effects of oceans and continents were those assigned by Humboldt, we should find in the western parts of America a colder climate than in the eastern parts; the reverse, however, is the case.

Now the climate of England is remarkably humid both in winter and summer. And this humidity is shown, not so much by the quantity of rain which falls, as by the frequent presence of large quantities of aqueous vapour in the atmosphere. Skies, even, which we in England consider clear, are overcast compared with the deep-blue skies of France or Italy. What the influence of these humid palls may be 'on the outflow of moral sentiments' which Humboldt considered to be so favourably influenced by transparent skies, I shall not here pause to inquire. It is clear, however, that the influence of our cloudy skies tends to modify the severity both of our winter and our summer seasons; and these benefits are so great that we may cheerfully accept them as more than a counterpoise for hypothetical injurious effects on 'the outflow of our moral sentiments' (whatever that may mean).

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[1] Gilbert White noticed long ago—apparently without understanding—the influence of a clouded sky on the temperature. 'We have often observed,' he says, 'that cold seems to descend from above; for, when a thermometer hangs abroad on a frosty night, the intervention of a cloud shall immediately raise the mercury ten degrees; and a clear sky shall again compel it to descend to its former gauge.'

I proceed to consider the actual variations presented in the course of a year in England. As some selection must be made, I shall select a series of observations which have been made at Greenwich during the present century. It will be gathered from the preceding pages that the range of temperature at Greenwich is at least not less than the average range of the British Isles. Greenwich, also, from its neighbourhood to London, and from the number and accuracy of the observations made there, is obviously the best selection that could be made. It must not be forgotten, however, that the climate of Greenwich is not the climate of the British Isles, and that careful observations made in other places have sufficiently indicated the existence of *local* peculiarities, which, therefore, it may fairly be assumed, characterise also the Greenwich indications.

In Fig. 3 the annual variations of mean diurnal temperature are represented graphically. The figure was formed in the following manner:—A rectangle having been drawn, each of the longer sides was divided into 365 parts, and a series of parallel lines joining every tenth of these divisions was pencilled in. The spaces separating these lines represented successive intervals of ten days throughout the year. The shorter sides were divided into thirty-three parts, and parallel lines drawn joining the points of division. Of these longer parallels the lowest was taken to represent a temperature of 32° Fahrenheit (*i.e.* the freezing point), and the others, in order, successive degrees of heat up to 65°. Then, from the Greenwich tables, which have been formed from the observations of forty-three years, the temperature of each day was marked in, at its proper level and at its proper distance from either end of the rectangle. Thus 365 points were marked in, and these being joined by a connected line presented the curve exhibited in Fig. 3. The lines bounding the months, and the lines indicating 35°, 40°, &c., Fahrenheit, were then inked in and the figure completed.

The resulting curve is remarkable in many respects. In the first place, it was to have been expected that a curve representing the average of so many years of observation would be uniform; that is, would only exhibit variations in its rate of rise and fall, not such a multiplicity of alternations as are observed in Fig. 3. And this irregularity will appear the more remarkable when it is remembered that the temperatures used as the Greenwich means are not the true average temperatures. They were obtained by constructing a curve from the true averages, and taking a curved line (the curve of Fig. 3, in fact) in such a way as to take off the most marked irregularities of the true curve of averages; or, to use the words of the meteorologist who constructed the Greenwich table of means, Mr. Glaisher, a curved line was drawn which passed through or near

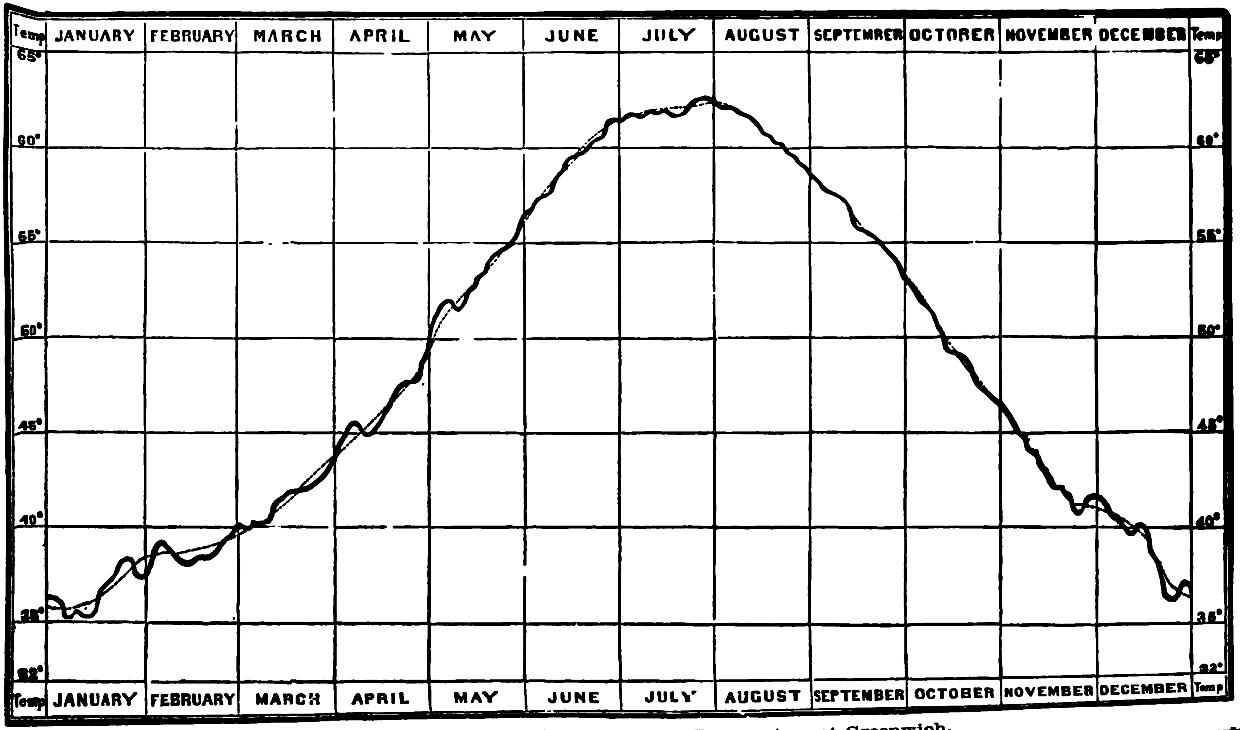

FIG. 3.—Annual Variation of Mean Diurnal Temperature at Greenwich.

all the points determining the true curve of averages, 'and in such a way that the area of the space above the adopted line of mean temperature was equal to that below the line.' Despite this process, the curve exhibits no less than fourteen distinctly marked maxima of elevation, and a much larger number of variations of flexure. The sudden variations of temperature at the beginning of February, early in April, and early in May, are very remarkable; they have their counterparts in the three variations which take place between the latter part of November and the end of the year, only these occur in much more rapid succession. The nature of the curve between June and August is also remarkable, as are the three convexities which are exhibited in the September, October, and November portions of the curve.

If we follow our leading meteorologists in taking the curve of Fig. 3 as representing the true annual climate of London, how are we to assign physical causes for the remarkable variations above indicated? Not easily, I take it. It were, indeed, as easy as inviting to speculate on cosmical causes; to follow Ertel, for instance, in assigning effects to those zones of meteorites which are known to intersect the earth's orbit, and others which may fairly be assumed to fall within or without that orbit. It may be, perhaps, that the recognised shooting-star periods have, some of them, their counterparts in heat-changes: but certainly the time has not yet come to pronounce a consistent theory of such effects. The evidence afforded by the Greenwich curve on this point is unsatisfactory, to say the least. The elevation at the beginning of January, and the marked irregularity in February, correspond to Ertel's views; so also the fact that large aërolites have frequently fallen in the first week in April, about the 20th of April, about the 18th of May, early in August,[1] about the 19th of October, and early in December, *seems* to correspond to elevations in the curve; while depression opposite the 12th of May might be referred to the intervention of the zone of meteors which causes the now celebrated November shower. But the negative evidence is almost equally strong. Where, for instance, is the elevation which one would expect, on Ertel's theory, in November? Also, if the cause of the observed irregularities were that suggested by Ertel, the curves for other countries in the northern hemisphere should exhibit similar irregularities on corresponding dates, which does not appear to be the case. In fact, if there really exist effects due to cosmical causes, these are not likely to be educed from observations of the variation of mean diurnal temperature, since it is clear that a cause of variation due to objects external to the earth could affect only the temperature of certain hours of one day or of several days. A cluster of meteors between the earth and the sun might diminish the mid-day heat; one external to the earth's orbit might increase the nocturnal temperature; and though in either case the mean diurnal temperature would be affected, yet it is obvious that the effect would be masked in taking the mean, or even that two or more opposing influences might cancel each other. If it could be shown that the curve for mid-day, or for midnight heat corresponded to the curve of mean heat, Ertel's theory would be overthrown at once; since, for its support it is necessary to show that depressions in the mean curve are due to mid-day loss of heat, and elevations to midnight gain of heat.

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[1] Reference is not made here to the August shooting-star shower, which takes place a week later than the epoch alluded to.

There are, however, terrestrial causes to which the irregularities of our curve (which irregularities, be it remembered, represent *regularly recurring irregularities* of heat) may be ascribed. For instance, there can be no doubt that our climate is considerably affected by the changes which take place in the Polar seas; and it may not unfairly be assumed that the processes by which different regions of Polar ice are successively set adrift (to be carried southward by the strong undercurrent known to exist in the northern Atlantic Ocean), take place at epochs which recur with tolerable regularity. And it may be that the irregularity of the rising as compared with the falling of the heat-curve is due to this cause; since the breaking-up of ice-fields and their rapid transport southwards would clearly produce sudden changes, having no counterpart in the effects due to the gradual process of freezing.[1]

It may well be, however, that the observations of forty-three years are not sufficient to afford the true mean diurnal temperature for a climate so variable as ours. Indeed, if the curves given by Kaemtz for continental climates be as accurately indicative of observed changes as that of Fig. 3, we must either accept such an hypothesis, or else assume that the English climate is marked by regularly recurring variations altogether wanting in continental climates; and it is to be noted that the regular recurrence of changes is a peculiarity wholly distinct from variability of climate, properly so termed, and seems even inconsistent with such a characteristic. It may happen, therefore, that the observations of the next thirty or forty years will afford a curve of different figure; and that by comparing the observations of the eighty or ninety years, which would then be available, many, or all, of the irregularities exhibited in Fig. 3 might be removed. In this case we might expect our climate-curve to assume the form indicated by the light line taken through the irregularities of Fig. 3. It will be observed that this modified curve exhibits but one maximum and one minimum. It is not wholly free, however, from variations of flexure. It presents, indeed, six well-marked convexities, and as many concavities; in other words, no less than twelve points of inflexion. The most remarkable irregularity of this sort is that exhibited near the end of November; and it is noteworthy that this irregularity is presented by continental climate-curves also. It has been ascribed by Ertel to the effect of the meteor-zone which causes the

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[1] Icebergs have been seen travelling southwards against a strong northward surface-current, and even forcing their way through field-ice in so travelling.

November shower. But as it is exhibited by the curves of *horary* as well as of *diurnal* means, while the meteor-zone cannot by any possibility affect the temperature of the earth's *following hemisphere*, and as, further, it does not correspond to the true date of the shower, this view may be looked upon as doubtful. The August curve occurring near the maximum elevation—where slow change was to be expected, is also well worthy of notice; as are the January and May flexures.

It will be noticed that nothing has been said of extreme heat or cold occasionally experienced in England. As such visits generally last but for a short time, their effects are not very injurious, save on the very weak, the aged, or the invalid. Corresponding to the passage of an immense heat-wave or cold-wave, there invariably occurs a sudden rise in the mortality-returns; but almost as invariably the rise is followed by a nearly equivalent, but less sudden fall showing conclusively that many of the deaths which marked the epoch of severest weather occurred a few weeks only before their natural time.

The weather during a part of the late winter was somewhat severer than our average English winter-weather. The thermometer, however, at no time descended below zero, as it did on January 3, 1854; and the diurnal mean did not descend at any time so low as 10° 7′, as it did on January 20, 1838. There is no foundation for the opinion, sometimes expressed, that our winter weather is changing. An examination of the columns in the Greenwich meteorological tables, shows that the successive recurrence of several mild winters is not peculiar to the last decade or two. The observations of Gilbert White, imperfect as they are compared with modern observations, point the same way.

Among severe, but short intervals of cold weather may be noted that which occurred in January 1768. The frost was so intense, says Gilbert White, 'that horses fell sick with an epidemic distemper which injured the winds of many and killed some; meat was so hard frozen that it could not be spitted, nor secured but in cellars; and bays, laurustines, and laurels were killed.'

White's account of the summer of 1783 will fitly close our sketch of British weather-changes. 'This summer,' he says, 'was an amazing and portentous one, and full of horrible phenomena; for besides the alarming meteors and tremendous thunder-storms that affrighted and distressed the different counties of this kingdom, the peculiar haze or smoky fog that prevailed for many weeks in this island, and in every part of Europe, and even beyond its limits, was a most extraordinary appearance, unlike anything known within the memory of man. By my journal, I find that I had noticed this strange occurrence from June 23 to July 20, inclusive, during which period the wind varied to every quarter, without making any alteration in the air. The sun at noon looked as blank and ferruginous as a clouded moon, and shed a rust-coloured ferruginous light on the ground and floors of rooms, but was particularly lurid and blood-coloured at rising and setting. All the time the heat was so intense that butchers' meat could hardly be eaten the day after it was killed; and the flies swarmed so in the lanes and hedges, that they rendered the horses half frantic, and riding irksome. The country people began to look with a superstitious awe at the red, louring aspect of the sun. Milton's noble simile, in his first book of "Paradise Lost," frequently occurred to my mind; and it is, indeed, particularly applicable, because, towards the end, it alludes to a superstitious kind of dread, with which the minds of men are always impressed by such strange and unusual phenomena:—

> As when the sun new risen,
> Looks through the horizontal misty air,
> Shorn of his beams; or, from behind the moon,
> In dim eclipse, disastrous twilight sheds
> On half the nations, and with fear of change
> Perplexes monarchs.'

Intellectual Observer, March 1867.

THE LOW BAROMETER OF THE ANTARCTIC TEMPERATE ZONE.

THE great difficulty presented by the science of meteorology lies in the intricate combination of causes producing atmospheric variations, and the impossibility of determining by experiment the relative efficiency even of the most important agents of change.

As Sir W. Herschel well observed, we are in the position of a man who hears at intervals a few fragments of a long history narrated in a prosy, unmethodical manner. 'A host of circumstances omitted or forgotten, and the want of connection between the parts, prevent the hearer from obtaining possession of the entire history. Were he allowed to interrupt the narrator, and ask him to explain the apparent contradictions, or to clear up doubts at obscure points, he might hope to arrive at a general view. The questions that we would address to Nature, are the very experiments of which we are deprived in the science of meteorology.'[1]

It is, therefore, but seldom in the study of this science that we meet with phenomena to which we can assign a definite cause, or which we can explain on simple principles. Even those marked phenomena, which appear most easily referable to simple agencies, present difficulties on a close investigation which compel us at once to recognise the efficiency of more causes than one. For instance, the phenomenon of the trade-winds, as explained by Halley, appears at first sight easily intelligible; but when we look on this phenomenon as a part merely—as indeed it is—of the marvellously complex circulation of the earth's atmosphere—when we come to inquire why these winds blow so many days in one latitude, and so many in another, or why they do not blow continually in any latitude—

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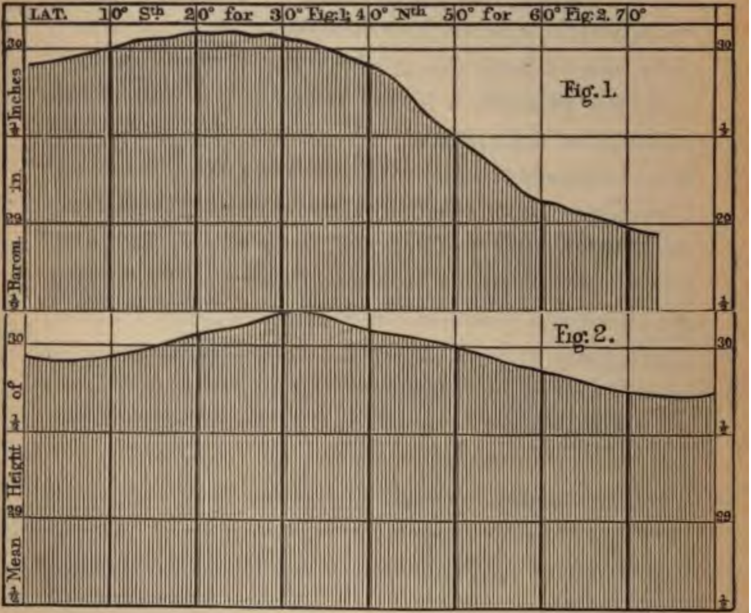
[1] Kaemtz's *Meteorology*.

when we consider the character of these winds as respects moisture and temperature, the variation of the velocity with which they blow, and of the quantity of air they transfer from latitude to latitude—we encounter difficulties which require for their elucidation the comparison of thousands of observations, or which baffle all attempts at elucidation.

There is, however, one atmospheric phenomenon—that which I have selected for the subject of this paper—which presents a grand simplicity, rendering the attempt at a simple solution somewhat more hopeful than is usually the case with meteorological phenomena. The discovery of this phenomenon formed one of the most interesting results of Captain Sir J. C. Ross's celebrated expedition to the Antarctic Ocean. He found, as the result of observations conducted during three years, that the mean barometric pressure varied in the following manner at the latitudes and places specified:—

South latitude		Height of barometer	Place
0°	0′	29·974 in.	At sea
13	0	30·016	—
22	17	30·085	—
34	48	30·023	Cape of Good Hope and Sydney
42	53	29·950	Tasmania
45	0	29·664	At sea
49	8	29·469	Kerguelen and Auckland Isles
51	33	29·497	Falkland Isles
54	26	29·347	At sea
55	52	29·360	Cape Horn
60	0	29·114	At sea
66	0	29·078	—
74	0	28·928	—

We see here a gradual increase of barometric pressure, from the equator to about 30° south latitude, and from this point at first a gradual diminution—so that in about 40° south latitude we find the same pressure as at the equator, and thence a more rapid diminution. The rate of change is illustrated graphically in Fig. 1, which represents the height of the barometer above 28½ inches for different southern latitudes. In the northern hemisphere there is a similar increase of pressure as we leave the equator, a maximum is there also attained in about latitude 30°; but from this point towards the poles there is a marked difference in



the rate of diminution of pressure in the two hemispheres. The following table by Schow is sufficient to indicate this:—

North latitude	Barometric pressure
0°	29·853 in.
10	30·002
20	30·004
30	30·069
40	30·006
45	30·011
50	29·943
55	29·960
60	29·835
65	29·623
70	29·722
75	29·863

There are minor irregularities in this table, due, doubtless, to local peculiarities, the arrangement of land and water being so much more complicated in the northern than in the southern hemisphere. Neglecting these (as in Fig. 2, which represents for the northern hemisphere the relations corresponding to those exhibited for the southern hemisphere in Fig. 1), we see that there is a much greater resemblance between the rise and fall of barometric pressure as we proceed northwards than as we proceed southwards. In fact, the curve is almost exactly symmetrical on either side of 30° north latitude to the equator on one side, and to latitude 60° on the other. From 60° the pressure continues to diminish for awhile, but appears to attain a minimum in about latitude 73°, and thence to increase. In the southern hemisphere, if there is any corresponding minimum, it must lie in a latitude nearer the south pole than any yet attained.

The most marked feature in the comparison of the two hemispheres is the difference of pressure over the southern and northern zones, between latitudes 45° and 75°. This is a peculiarity so remarkable, that for a long time many meteorologists considered that the observations of Captain Ross were insufficient to warrant our concluding that so important a difference really exists between the two hemispheres. But not only has Captain Maury—from a comparison of 7,000 observations—confirmed the results obtained by Ross, but, in meteorological tables published by the Board of Trade, the same conclusions are drawn from 115,000 observations, taken during a period of no less than 13,000 days. In fact, it is now shown that the difference is yet greater than it had been supposed to be from the observations of Captain Ross. From a comparison of observations made in the Antarctic Seas with those of Captain Sir Leopold McClintock, it appears that the average difference of barometric height in the northern and southern zones, between latitudes 40° and 60°, is about one inch. Figs. 1 and 2 exhibit a relation midway between these later results and those tabulated above.

Assuming an average difference of only three-quarters of an inch in the northern and southern zones, between latitudes 40° and 60°, let us consider what is the difference of pressure on these two zones of the earth's surface. The area of either zone is 21,974,260·5 square miles, and the pressure on a square mile due to a barometric height of three-quarters of an inch is about 670,000 tons, therefore the pressure on the northern zone, between the latitudes named, exceeds the pressure on the southern zone by no less than 14,500,000,000,000 tons. Including all latitudes within which there has been ascertained to be a difference of barometric pressure in the two hemispheres, we shall probably be within the mark if we say, that the atmospherical pressure on the northern hemisphere is 20,000,000,000,000 tons greater than the atmospherical pressure on the southern hemisphere.

Such a peculiarity as this may almost deserve to be spoken of in the terms applied by Sir J. Herschel to the distribution of land and water upon our earth, it is '*massive* enough to call for mention as an *astronomical feature*.' I propose to examine two theories which have been suggested in explanation of this feature of the earth's envelope. These theories are founded on *local* peculiarities, and the feature considered appears as a *dynamical* one—that is, as a peculiarity resulting from states of motion in the aërial envelope. I shall endeavour to establish a theory founded on a consideration of the earth's mass *as a whole*, and presenting the atmospheric feature in question as a *statical* one.

The first theory I have to notice is one founded on the configuration of land and water upon the northern and southern hemispheres of the earth's globe. In the northern hemisphere, and more especially in that part of the northern hemisphere in which barometrical observations have been most persistently and systematically conducted, there is much more land than in the southern hemisphere. Now barometrical observations are referred to the sea-level, and observations made in Europe and America may be considered as referred to the level of the northern parts of the Atlantic Ocean. It is argued that the North Atlantic, compared with southern oceans, is little more than 'a large lake, having elevated banks east and west.' 'Practically, the air there is a portion of the solid globe, so that the unconfined air will rest upon and rise above the former, as if it were solid and a portion of the earth; so that the altitude of the air over the North Atlantic will be increased some hundreds of feet, and the barometer at the sea-level will be pressed upon, not only by the free air clear of the earth's banks, but also by the air confined in the basin, much as if the air were at the bottom of a mine.'[1]

Presented in the above form, the theory that the higher northern barometer is due to the contour of the northern hemisphere scarcely deserves serious comment. To speak of the confined air of the North Atlantic Ocean is surely unreasonable. An ocean 2,000 miles across, swept by more frequent storms than are experienced in any other part of the globe, cannot be very aptly compared to 'the bottom of a mine.' An inelastic fluid flowing steadily over a rugged surface shows no trace, or but the slightest trace, of the nature of that surface, by any variations of its own level. But it is still less conceivable that an elastic fluid should be influenced in the manner suggested. In fact, if this happened, we should no longer be enabled to determine the heights of mountains by barometric observations; for according to the theory the air should extend to a greater height above mountains than above plains; and as regards comparison between a barometer at the foot of a mountain and one at the summit, we might argue that the barometer in the valley, compared with a barometer at the same level in a plane district, 'is pressed upon, not only by the air clear of the mountain tops, but also by the air confined within the valley,' so that the altitude over the valley is greater by some hundreds of feet than the altitude over a plain at the same level as the valley; and thus, before we could determine the height of the mountain above the level of the plain, we should have to determine the exact effects due to the confinement of the air in the valley. We know that, on the contrary, the average barometric pressure in the most confined valley does not differ appreciably from the average pressure over the most widely extended plain at the same level.

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[1] From a letter addressed to the editor of the *Athenæum* by Dr. H. Muirhead.

We may, however, reasonably inquire whether the presence of continents in the northern hemisphere might not operate in another manner. If we place any mass within a vessel containing fluid, it is clear that we increase the fluid pressure over every point of the vessel's bottom, because this pressure depends wholly on the depth of the bottom below the level of the fluid, and the level rises when any solid substance is placed within the vessel. Now if we suppose a globe covered all over by water to be surrounded by a perfectly uniform atmospheric envelope, the mean pressure of this envelope at the water-level would certainly be increased if continents were supposed to be raised in any manner above the surface of the water; and if the atmosphere over one half of such a globe were supposed to be prevented in any way from mixing freely with the atmosphere over the other half, then it is clear that the mean pressure at the water-level would be greatest on that half-globe over which the most extensive and highest continents had been raised. On the assumption, then, of some such arrangement over our own earth—an arrangement, that is, which should prevent the northern air from mixing with the southern—one might see in the northern continents a true cause of increased barometric pressure at the sea-level of the northern hemisphere.

We have, however, not only no evidence that such an arrangement exists, but very strong evidence of an atmospheric circulation which carries air from hemisphere to hemisphere, and mixes in the most intimate manner the whole mass of gases which form the earth's atmospheric envelope. The whole question of the circulation of the air is investigated in Maury's interesting work on the *Physical Geography of the Sea*, and he appears to establish in the most convincing manner the interchange of air between the northern and southern hemispheres.

And even if we could assume that the atmospheric covering of any portion of the earth's surface was in any way prevented from passing freely to other regions, yet the cause assigned would be inadequate to account for the difference of barometric pressure actually existing between the two hemispheres. All the land above the sea-level in the northern hemisphere, if uniformly distributed over the surface of that hemisphere, would be raised to a height of less than 200 feet above the present sea-level, and the actual difference of level corresponding to the observed difference of barometric pressure is more than four times as great.

Passing over this theory as neither consistent with the known laws regulating the motions of elastic fluids, nor sufficient even if the consideration of those laws were neglected, we come to the theory suggested by Captain Maury—a theory deserving of much more attentive consideration. I shall quote his own words, as the fairest method of presenting his theory; after stating the observed difference of barometric pressure in the two hemispheres, and mentioning the expulsion of air from the northern hemisphere as the cause of this difference, he writes:—'To explain the great and grand phenomena of nature, by illustrations drawn from the puny contrivances of human device, is often a feeble resort, but nevertheless we may, in order to explain this expulsion of air from the watery south, where all is sea, be pardoned for the homely reference. We all know, that, as the steam or vapour begins to form in the tea-kettle, it expels air thence, and itself occupies the space which the air occupied. If still more heat be applied, as to the boiler of a steam-engine, the air will be entirely expelled, and we have nothing but steam above the water in the boiler. Now at the south over this great waste of circumfluent waters, we do not have as much heat for evaporation as in the boiler or the tea-kettle; but, as far as it goes, it forms vapour, which has *proportionately* precisely the same tendency that the vapour in the tea-kettle has to drive off the air above, and occupy the space it held. Nor is this all. This austral vapour, rising up, is cooled and condensed. Thus a vast amount of heat is liberated in the upper regions, which goes to heat the air there, expand it, and thus, by altering the level, causes it to flow off.'

The theory thus divides itself into two parts: we have first the expulsive effects due to the vapour raised from southern oceans; and, secondly, the expansive effects due to the liberation of heat as the vapour is condensed. Now I would, in the first place, submit that we cannot assign to the second cause the effects here considered. The amount of heat liberated as the vapours rising from the southern ocean are condensed is undoubtedly great, but it cannot be more than the equivalent of the amount of heat rendered latent as the vapours are formed, and therefore the expansive effects due to the liberation of heat cannot be greater than the contrary effects due to the prior imprisonment of heat. It is quite true, and has been accepted as the undoubted explanation of many climatic effects, that if vapour be raised in one place and condensed over another, then the temperature of the air over the latter place is raised. But when we have to consider a phenomenon extending over a zone twenty or thirty degrees in width, we cannot argue in this manner. Nay, it is *necessary* to the force of Maury's second cause that the condensation of vapour should take place over the very zone in which the vaporisation is proceeding. To assign similar effects to both processes, is to require that the winding up and the loosening of the spring should take place in the same direction.

Whatever effects, then, are due to the constant evaporation going on in the southern hemisphere, must not be derived from changes of temperature. So far as these are effective at all, they must depend on the excess of evaporation over condensation (since the excess cannot possibly lie the other way), and therefore represent diminution of heat or increase of pressure, the contrary effect to that we have to account for. We have, therefore, only to consider the first cause mentioned by Maury; that is, the expulsive effects due to the formation of aqueous vapour.

At first sight, this process of expulsion appears simple enough, and seems further to coincide with many well-known phenomena. The theory supposes that over a wide zone of the southern hemisphere aqueous vapour is continually rising; that as it rises it displaces in part the heavier air over these regions; and that equilibrium being thus disturbed, the excess of air flows off continually towards the equator. Now we know that the prevailing surface-winds over that zone of the southern hemisphere in which the barometer exhibits the peculiarity we are considering, blow from the equator; that is, they tend to sweep the lower strata of the atmosphere towards the south pole. They therefore tend to increase the quantity of humid air in high southern latitudes. We know also that the prevailing upper currents over the southern zone we are considering blow towards the equator. They tend, therefore, to carry the drier portion of the air towards the equator. All this seems in accordance with Maury's theory, and indeed if the prevailing upper and lower currents flowed in directions contrary to those indicated, the theory would fall at once.

Again, although we find no evidence in barometric pressure over the south tropical zone of that increase which Maury's theory would lead us to expect (since the surplus air is carried first to this zone), yet it might be argued that the surplus is so distributed as to appear in another way. It is evident that if the atmospheric envelope normally appertaining to the southern hemisphere were, through the effects of the causes assigned by Maury, increased in extent, this increase might show itself, not in an increase of pressure over the south tropical zone—that is, not in an increase of *height* there—but in the extension of the surplus atmosphere into the northern hemisphere. This would be shown by the extension of the southern trade-winds to or beyond the equator, so that the (so-called) equatorial zone of calms should lie north of the equator. As this is really the position occupied by the belt of calms, Maury's theory appears to gain additional force by the coincidence.

Another argument may be drawn from the analogy of the low barometer in moist weather. In fact, it is well known that Deluc explained this phenomenon in a manner precisely accordant with the views expressed by Maury.

Despite the apparent force of these arguments, and others that might be adduced, it will not be difficult, I think, to show that neither is Maury's theory consistent with known physical laws, nor (passing over this objection) is the theory *sufficient* to account for the grand phenomenon under consideration.

It is quite true that a volume of aqueous vapour weighs less than an equal volume of air; it is equally true that a volume of moist air weighs less than an equal volume of dry air *at the same tension*. But water, quietly evaporating in the open air, does not displace the air, but penetrates into its interstices, according to the well-established law regulating the mixture of vapours. The aqueous vapour which thus intimately mixes itself with the air produces no effect whatever, either by its weight or by its elasticity, on the movements of the atmosphere. The experiments of Gay-Lussac, Dalton, and others, have long since proved that the actual effects of the quiet evaporation of water are those here described. It is on this account that Deluc's hypothesis in explanation of the fall of the barometer when the air is moist is now no longer accepted. It has been shown that the observed fall is not due to the moistness of the air, but to increase of temperature. Hot winds bring (in Europe) moist air, and thus moist air and a low barometer are found to be coexistent phenomena. But they are not in the relation of cause and effect. In fact, in New Holland, where hot winds bring dry air, we find the barometer low when the air is dry.

It follows from what has just been said of the manner in which aqueous vapour associates itself with air, that atmospheric pressure is increased instead of diminished by the process of quiet evaporation, since the weight of the vapour is added to that of the air. Therefore, all things being equal, we should expect to find the barometer higher in the southern or watery hemisphere than in the northern.

It might seem unnecessary to consider Maury's theory further, but as some doubts may still remain whether some process of the kind conceived by him may not take place,[1] I proceed to consider the *efficiency* of such a process to account for the great phenomenon we are dealing with.

It must be remembered, in the first place, that the theory requires that there should be a greater volume of mixed air and vapour over the southern temperate zone than there is in the corresponding northern zone, otherwise there would not be that continual overflow towards the equator which is required by the theory. So far as it goes, this increment of volume implies an increment of weight. The increase of volume is more than compensated (in theory) by diminution of specific gravity, but it must be held in mind that the increase of volume has to be accounted for by the theory *as well as* the difference in barometric pressure.

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[1] In fact, Sir J. Herschel, in his work on Meteorology, assigns as a cause of the low barometric pressure near the equator, compared with that near the tropics, a process similar to that conceived by Maury, only depending on the excess of heat near the equator. I cannot but agree with those meteorologists who consider that the notion of any appreciable *uplifting* of the air by the rising vapour of water is a mistaken one. But whether it be so or not, it is evident that Herschel's view would require a regular increase of pressure from the equator to the antarctic pole, and therefore is opposed to Maury's explanation

Again, the theory requires that the upper regions of air should be dry, for it is the upper air that is carried towards the equator; and if this air were moist, we should no longer have the different proportions of moist and dry air which are required by the theory. We *must* have an aggregation of moist air in high southern latitudes, and of dry air towards the equator.

Again, we must call to mind that one-half of the northern hemisphere is covered by water, and a part of the southern hemisphere is not so covered, so that the effects suggested by Maury are (1) not peculiar to the southern hemisphere, nor (2) do they prevail over the whole of that hemisphere.

Lastly, we must remember that the process conceived by Maury must be wholly or principally a diurnal process, and so can only take place (on an average) over one half of the southern zone at any one time.

All these considerations tend to diminish very importantly the efficiency of the cause assigned by Maury. Let us, however, consider what is the maximum value that efficiency could have if all these circumstances were neglected. We shall see that even in this case, which assigns an efficiency at least three or four times as great as would be consistent with actual facts, we shall still find the cause assigned by Maury inadequate to the production of the phenomenon under consideration.

The greatest weight of aqueous vapour which is ever present in a given volume of air is equivalent to about one-sixtieth part of the weight of the air. Now, if we suppose the barometer at thirty inches, and the whole column of air above the barometer to be impregnated with air in the above-named proportion—a view very favourable to the theory, since the cold of the upper regions of air largely diminishes the proportionate weight of aqueous vapour—it is clear that one-sixtieth part—or half an inch—of the barometer's height is due to the presence of aqueous vapour. Now, at mean tensions the specific gravity of aqueous vapour is about three-fifths of the specific gravity of air, so that the proportion of one-sixtieth part of weight corresponds to a proportion of one-thirty-sixth part of volume; in other words, our column of air owes one-thirty-sixth part of its height to the presence of aqueous vapour. If we suppose this thirty-sixth part to flow off—not from the upper regions only, but in such a manner that one complete thirty-sixth part of the volume of the column should pass off—then, instead of standing at a height of thirty inches, the barometer would stand at a height of $29\frac{1}{6}$ inches, less by only one-third of an inch than the height of $29\frac{1}{2}$ inches due to the dry air alone.

Now we cannot, in accordance with Maury's theory, legitimately add the five-sixths of an inch of barometric pressure to the height of the barometer under a neighbouring column. For we have no evidence to show that the air assumed to be expelled from the southern temperate zone is heaped over the southern tropical zone; on the contrary, we have a barometer in the latter zone not quite so high even as the barometer in the corresponding northern zone. Therefore if air is expelled in the manner supposed by Maury, it must be distributed over a very much greater portion of the globe's surface than it had been expelled from. Hence, returning to our imaginary column of air, but a small fraction of the five-sixths of an inch due to overflow must be added to the barometer under a neighbouring air-column. The latter barometer originally at $29\frac{1}{2}$ may be fairly assumed to rise at most to about $29\frac{5}{6}$ inches. We have, then, a difference of $29\frac{5}{6}$—$29\frac{1}{6}$ inches, or two-thirds of an inch; so that despite all the opposing considerations we have neglected, we still have a difference less by one-third than that for which we have to account; and, indeed, so far as the comparison between the northern and southern temperate zones is concerned (and this is the true question at issue), we are only entitled to consider the third part of an inch lost by overflow, as the true measure of the efficiency of this cause.

So far as I am aware, the theory I am about to present in explanation of the phenomenon of a low antarctic barometer is original. It is sufficiently simple;—

perhaps, if we remember how very seldom physical phenomena admit of a simple explanation, one may say that the theory labours under the disadvantage of simplicity.

It is obvious that the centre of gravity of the solid portion of the earth's globe lies somewhat to the south of the centre of figure. This arrangement has long been accepted as the explanation of two remarkable geographical features—the prevalence of water over the southern hemisphere, and the configuration of nearly all the peninsulas over the whole globe. Whether or not those parts within the antarctic regions which have not yet been explored, are occupied by land (chiefly) is a question which has little more bearing on our views respecting this point than has the counter question—whether the unexplored north-polar regions are or are not occupied by a north-polar ocean.[1] Supposing these arrangements to exist, it is evident that they form mere local peculiarities. The general tendency of water towards the southern hemisphere is very obvious, and, so far as I am aware, no other explanation

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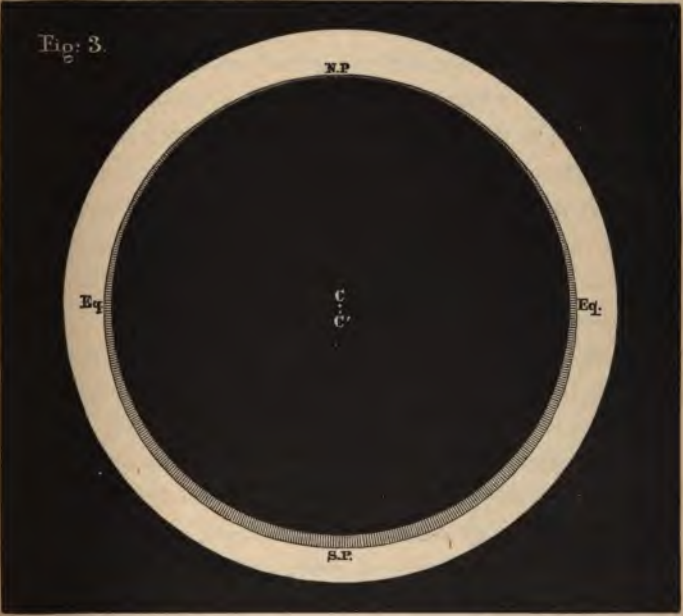
[1] Captain Maury holds the affirmative on both points. I have already had occasion to discuss in these pages his theory of a *tidal* north-polar ocean, and I think the theory cannot be maintained. But the theory of a polar ocean communicating with the Atlantic and Pacific is a sufficiently probable one. The theory of an antarctic continent is hardly in the same position, since antarctic explorations have given us but faint indications, here and there, of the habitudes of the south-polar regions. But I may note, in passing, a very singular argument used by Captain Maury in favour of the existence of such a continent. He states it as a physical law that land is scarcely ever antipodal to land; 'therefore,' he says, 'since the north-polar regions are probably occupied by a vast ocean, the south-polar regions are probably occupied by a vast continent.' He seems to forget that it by no means follows that because land is seldom antipodal to land, water should seldom be antipodal to water. Since the extent of water is nearly three times that of land, it is absolutely necessary that nearly two-thirds of the water should be antipodal to water. The supposed peculiarity that nearly all the

Fig: 3.



of the peculiarity has ever been offered than that founded on a slight displacement southwards of the earth's centre of gravity. If, then, C is the centre of the black circle in Fig. 3, representing the solid part of the

land is antipodal to water (one twenty-seventh only being antipodal to land), is in reality no peculiarity at all. It would have been far more singular if any large proportion of the land (which occupies little more than one-fourth of the globe) had been antipodal to land.

earth, the centre of gravity of this part lies (in the Fig.) slightly below C—between C and C′, let us suppose.

Now we see that, owing to the slight displacement, the watery envelope of the earth tends southward. If the earth were a perfectly uniform spheroid, it is clear that there would be a tendency to some such arrangement as is represented (on a greatly exaggerated scale) in Fig. 3, in which the shaded part represents the sea —that is, a shell of water, thicker towards the south, would surround the solid earth. For our present purposes it is sufficient to consider this supposed arrangement, as minor inequalities of the earth's surface-contour have clearly nothing whatever to do with the phenomenon we are considering.

Let C′ be the centre of the spheroid which bounds the earth's fluid envelope. Then it is very clear that if this envelope were of the same specific gravity as the solid portion of the earth, the centre of gravity of the entire mass would lie very near C′, but slightly south of that point, on account of the slight southerly displacement of the centre of gravity of the solid portion. But when we consider that the specific gravity of the fluid envelope is less than one-fifth of that of the solid globe, it is perfectly clear that the centre of gravity of the entire mass will not be so far south as C′. For, of the entire mass, the northern half is the heavier, and therefore the centre of gravity must lie north of the centre of the entire mass—that is, north of C′. In fact, it must lie much nearer to C than to C′.

Thus, the centre of gravity of the solid globe, and that of the entire mass, solid and fluid, both lie between C and C′. Now it is evident that the central point, about which the earth's atmospheric envelope tends to form itself as a spherical or spheroidal shell, is the centre of gravity of the entire solid and fluid terrestrial globe—that is, is a point north of C′. Therefore, precisely as the effect of the fluid envelope collecting itself centrally about a point *south* of C is to cause the mean depth of water to be greatest in the *southern* hemisphere, so the fact that the atmospheric envelope collects itself centrally about a point *north* of C′ should result in giving a greater mean depth of air (*referred to the sea-level*) over the *northern* hemisphere. This arrangement is represented in Fig. 3, in which the unshaded part is supposed to represent the atmosphere.

I have endeavoured to make the above explanation of my theory in explanation of the low antarctic barometer as complete and exact as possible; but there is another way of presenting the theory, which, though less complete, may appear clearer to some minds:—

Variation of mean barometric pressure, as we proceed from one place to another, may be due either to a variation of circumstances of heat, moisture, and other like relations, or to difference of level. Maury's explanation assigns to the low antarctic barometer a cause or causes falling under the former category. My theory amounts to the supposition that the low barometer is due to an absolute difference of level. I say that the sea-level, to which we refer barometric pressure, is *not* a just level of reference when atmospheric pressure over the whole globe is the subject of inquiry, because the southern seas stand out to a greater distance than the northern seas from the true centre of gravity of the earth's solid and fluid mass.

Assuming my theory to be correct, we have a means —rough, it may be, but not uninstructive—of determining the displacement of the centre of gravity of the earth's solid mass from the centre of figure. For, accepting one inch as the difference of barometric height at the two poles, it is easily calculated that this difference amounts to a difference of level of about 850 feet. In other words, the surface of the water at the south pole lies farther than the surface of the water at the north pole from the centre of gravity of the entire fluid and solid globe, by about 850 feet. Hence this centre of gravity must lie about 425 feet north of C′ (which is the centre of the bounding surface of the water). Now, it is evident that both the centre of gravity of the entire fluid and solid mass, and that of the solid mass, must lie much nearer to C than to C′. Hence both these centres of gravity lie considerably within 400 feet of C, and C′ lies considerably within 825 feet of C. Thus the centres of figure and the centres of gravity of the earth's solid mass, and of the entire fluid and solid mass are collected within a space less than one-eighth of a mile in length—a distance almost evanescent in comparison with the dimensions of the earth's globe.

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